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THE PSYCHOLOGICAL REVIEW

THE INVESTIGATION OF THE OLFACTORY QUALITIES[1]

BY FORREST L. DIMMICK
Hobart College

Since Henning's presentation of his 'Geruchsprisma'[2] there have been three attempts to evaluate the theory experimentally from a psychological point of view. The first[3] was the determination of the classification of some 75 odors for the purpose of obtaining a set of stimuli that could be used with a reasonable degree of certainty to demonstrate the six classes and their intermediates. This experiment, obviously, is not a test of the theory, but the low degree of positive confirmation of Henning's findings and the wide scatter of the results indicate the need for a more careful description of the conditions, the stimuli, etc., under which results such as Henning reports are to be obtained. The second, a critical study, was made by MacDonald.[4] He set out to test both the psychological and the chemical aspects of the theory. He began by adopting standards for the several corners of the prism which, both qualitatively and chemically, were in best accord. With these standards he compared, by a method of paired comparison, 18 other carefully chosen

[1] Some of the experimental work reported in this article was carried on at the Univ. of Michigan. Thanks are due to Miss Dorothy Greenwald and to Dwight M. Steere for their assistance.

[2] Henning, H., 'Der Geruch,' Leipzig, 1916, pp. 533.

[3] Dimmick, F. L., 'A Note on Henning's Smell Series,' *Amer. J. Psychol.*, 1922, 33, 423 *ff.*

[4] MacDonald, M. K., 'An Experimental Study of Henning's System of Olfactory Qualities,' *Amer. J. Psychol.*, 1922, 33, 535 *ff.*

olfactory stimuli and thus determined their qualitative relations to them. He failed, first, to verify the Henning prism to a degree of more than 30 per cent to 40 per cent and he found that his trained Os "could . . . not give judgments consistent with themselves or with one another." He then modified his procedure and restricted it to a single face of the prism but with no material change in his results. He concludes that while "Henning's theories represent an advance, a first approximation to the truth, they cannot . . . be applied rigorously . . . ." In addition he points out the inadequacy of Henning's chemistry. Findley [5] repeated MacDonald's experiment with certain improvements of method and with stimuli chosen from our list because of their agreement with Henning's theory. Her work verifies MacDonald's findings and she concludes that "judgments of the qualitative similarity of odors is very variable."

So far as we can judge from these investigations, the 'Geruchsprisma' is as yet far from being established. They agree with it partially and in a very general way but fail when precision is demanded. When we turn to Henning's presentation of his theory we find that the results that we have just reviewed are not surprising. Henning's procedure falls into two parts. In the first place he endeavored to bring out the group similarities among odors. The stimuli were presented one at a time and the Os were instructed: "Die Geruchsqualitäte soll genau einfasst und beschrieben werden. Es liegt mehr daran, das ein Urteil gebildet wird, als dass die Exposition rasch verläuft. Nicht überhetzen, Zeit lassen!" [6] In the course of the descriptions, the Os were prone to make certain confusions among the odors. These confusions were taken to indicate that the confused odors belong to the same qualitative group. Henning gives six criteria for the group similarities. (1) "Der dargebotene Geruch war falsch benannt." (2) "Die Kennzeichnung eines Geruches wird aus der Ähnlichkeit mit einem bereitsexpo-

[5] Findley, A. E., 'Further Studies of Henning's System of Olfactory Qualities,' *Amer. J. Psychol.*, 1924, 35, 436 *ff.*

[6] Henning, H., *op. cit.*, p. 51.

nierten Geruche hergeleitet." (3) "Der Geruch wird weder richtig noch falsch benannt, vielmehr kann nur eine Ähnlich keit festgestellt werden." (4) "Bei einem Geruche wird fälschlich durch Wiedererkennungsprozess ein vorher gebotener anderer aber ähnlicher Geruch als derselbe bezeichnet." (5) "Der Geruch kann *nicht* speziell benannt werden, vielmehr nur die Klasse." (6) "Nicht direkt abhängig von der Beurteilung des dargebotenen Geruches wirdt öfters eine Ähnlichkeit oder Unähnlichkeit mehrer bereits exponierter Geruche festgestellt." [7] The argument [8] for these criteria is that in as much as we cannot conceive of two totally different qualities of smell being confused, the mistaking of one odor for another must indicate that the two belong, at least, to the same olfactory class. This part of his investigation consisted of 2747 [9] judgments by 15 Os. However, two Os gave more than half of them. The only statistical treatment that we can find is a table of the distribution of the relative correctness or falsity of the judgments of the qualities of the odors. It seems, then, that the particular qualitative similarities or classes among odors that stand at the corners of the prism are based not upon any sort of statistical proof but upon some general trend that appeared to Henning in the qualitative descriptions of his two trained Os.

Finally, a careful examination of Henning's criteria of 'richtig,' 'falsch,' and 'ähnlich' [10] and of the examples of these respective judgments makes it appear possible that Henning, himself, has not fully avoided the error, against which he warns us, of dealing with 'Gegenstandsgerüche' instead of 'reine Gegebenheitsgerüche.' For example, 'Gewürz des heute gebackenen Weinachskuchen,' [11] bears reference to the object rather than to the olfactory quality. Such criteria do not hold in vision, for instance; a red-green blind individual will readily distinguish red and green objects

[7] *Ibid.*, pp. 52–61.
[8] *Ibid.*, p. 52.
[9] *Ibid.*, p. 61.
[10] *Ibid.*, p. 63.
[11] *Ibid.*, p. 63.

and a perfectly color-competent person will misname colors. Indeed, the very names which Henning chose for the classes of odors refer to objects whose smells are only incidental, *e.g.*, 'würzig,' 'blümig,' 'fruchtig,' etc.

Henning's second problem, after he had established to his own satisfaction the classes of odors, was to discover the serial interrelation among groups. He describes his method thus. "Die unwissentliche Versuchsanordnung war in der Folge so, das unmittlebar hinter einander eine Zahl von Gerüchen geboten wurde; sie waren auf die Qualität zu prüfen und zu ordnen, ohne das der Nahme reproduziert zu werden brauchte. Die Vp. sollte diejenigen Gerüche herausstellen, die nicht in die Reihe passten. Als Anhaltspunkt galt bei der Zuordnung der zuerst gebotene Geruch, dann die ihm beigesellsten."[12] Here, again, there is no attempt to present a statistical proof of the serial interrelations of the odors. The reports of one O (Vp. E) are given as typical if not conclusive. Obviously, then, no such statistical procedures as those of MacDonald and Findley can be expected to give a high degree of correlation.

The second edition of 'Der Geruch'[13] gives no additional information about the smell prism. The promised 'genaues Model des Geruchskörper mit allen erreichbaren Reichstoffen'[14] is not forthcoming. Henning has been unable to obtain the necessary substances. The prism has receded from its important position in the discussion and has been relegated to a later chapter. The greater part of the data upon which it is based has been omitted entirely. In the 'Anhang'[15] Henning replies to some of his critics. The chemists (with whom he includes MacDonald!) have failed to appreciate that the classification is not chemical but psychological, and that the same substance may, under different conditions, give odors that fall upon very different parts of the prism. "Da keine eindeutige Reizgebundenheit und Qualitätbestimmtheit existiert, bringt eine Ordnung der

[12] *Ibid.*, p. 82.
[13] Henning, H., 'Der Geruch,' 2te Auflage, Leipzig, 1924, pp. 434.
[14] Henning, H., *op. cit.*, 1ste Auflage, p. 81.
[15] Henning, H., *op. cit.*, 2te Auflage, p. 423.

objectiven Reize keineswegs *eo ipso* die zugehörigen Empfindungen in eine adäquate Ordnung." [16] This fact, however, has not decreased the positiveness with which the theoretical correlation between olfactory group and atomic structure is presented. The discrepancies between smell stimulus and smell quality are more fundamental than this. ". . . Da ein und derselbe Reichstoff in unterschiedlichen Konzentrationen ganz anders reicht, Residuenwirkungen den Qualitätseindruck umändern, und zahlreiche Faktoren bedingen, . . . derselbe chemische Stoff einmal anders reicht wie das nächste Mal." [17] On this basis, all failures to verify the prism are disposed of. "MacDonald findet an seinem drei Vpn., das eine und derselbe chemische Reichstoff bald so, bald anders reicht, und wähnt damit—mein Geruchsprisma widerlegt!" [18] Must it, then, be accepted on faith?

Experiment I

"Zunächst wollen wir zufrieden sein, wenn wir die ungeheure Manningfaltigkeit der Gerüche mit gleicher Genauigkeit ordnen können, wie es bei den Farben gelungen ist." [19] Henning implies that his results meet this standard. If we apply the criterion experimentally we should expect that any group of odors can be placed on the prism (or in relation to the standard corner odors) with the same accuracy and the same discrepancies that a similar group of colors can be placed on the color pyramid (or in relation to the standard corner colors). We attempted to test this assumption by classifying a group of odors and a group of colors under the same experimental conditions. We obtained 100 odors and 100 colors and required 30 Os to indicate the resemblance of every one of the odors to the six standard odors,—those that represented best, as nearly as we could judge from Henning's exposition, his six olfactory classes,—and 30 Os to classify every one of the colors with reference to the color standards,—red, yellow, green, blue, black and white. Class names were

[16] *Ibid.*, p. 424.
[17] *Ibid.*, p. 423.
[18] *Ibid.*, p. 424.
[19] Henning, H., *op. cit.*, 1ste Auflage, p. 80.

never used. The procedure in both cases was determined by the necessities of olfactory stimulation. Here a strictly successive presentation is necessary. Therefore, in the color experiment the Os were presented only one color—standard or variable—at a time. No simultaneous comparisons were permitted.

TABLE I

	Per Cent Agreement with Theory	Per Cent Agreement among Os
Colors	95	95
Odors	40	25

The results as summarized in Table I demonstrate clearly that the two problems of classification are not comparable. The disagreements in the placing of the colors either with theory or among Os are negligible. The few cases of confusion that did occur were in the categories of black and white. The odors, on the other hand, gave only 40 per cent of cases that could be taken to agree in any sense with Henning's theory. In the remaining cases every odor was placed, in a manner resembling chance, under at least 5 or 6 headings. The agreement among Os was even less. Every possible combination of the 6 qualities 1, 2, 3, and 4 at a time was ascribed to every odor with very few exceptions (11 per cent). Henning's statement that his arrangement of odors is comparable with that of colors is not borne out by our experimental findings.

EXPERIMENT II

In our first experiment we adopted standards for the olfactory classes and we argued from the statistical agreement and disagreement of the judgments of a large group of Os. Both of these procedures are deviations from Henning's methodology. In the present experiment we followed Henning's second method exactly. We selected a limited set (14) of odors from the region for which Henning gives the largest number of examples, that is, from the Fruity-Fragrant region through the Fragrant, the Fragrant-Spicy, and the

Spicy to the Resinous-Spicy region. The stimuli were contained in 14 identical opaque bottles which bore no identifying marks visible to the Os. They were instructed: "Take every odor, one at a time, smell of them by holding the mouths of the bottles about 2 cm. below and equally distant from the two nostrils, and place them in relation to one another according to their qualitative resemblances." They were advised, further, that the series might be linear or multidimensional or that there might be more than one series. A second presentation and a serial arrangement were made one week after the first. We secured 23 such pairs of serial arrangements.

Findley, under the direction of Professor Boring in the Harvard laboratory, carried out the same experiment with 10 Os, following the same procedure and using the same stimuli. Her set of results and ours differ no more from each other than each does within itself. The correlation between the pairs of serial arrangements range from 0 per cent to 90 per cent, with an average of 58 per cent. (There are no minus correlations because the sign depends merely upon the end which is taken as a starting point of the series. A minus coefficient would have been plus if only we had put the other end first for purposes of correlation. There were no fixed first and last places for the Os; the two were interchangeable.) The forms of the series differed from O to O and for the same O as greatly as did the orders. Linear forms were most frequent (59 per cent), but there were numerous closed forms such as triangles, squares, and circles (28 per cent), and there were several sets of disconnected and radiating lines (13 per cent). We have secured serial arrangements but we should not be justified in using any one or any group of them as a positive determination of a qualitative series of odors as Henning does the arrangements of his Vp. E.

Experiment III

Our results this far, while not antagonistic to Henning's, are not confirmatory. The reason for this fact may lie in the intrinsic difficulty of judging odor qualities, or it may depend

upon the instability of the odor stimuli, or it may be due to the descriptive inadequacy of our present olfactory categories. In our next experiment we endeavored to test these three possibilities in some degree. We tried to rule out as much of the intrinsic difficulty of judging in terms of the qualities of odors as is dependent upon the conditions of presentation, upon the judgmental problem set the O, and upon the training of the O in the making of such judgments.

First we devised an artificial nostril which gave us adequate control over the presentation of the stimuli. This consists of a metal chamber that fits snugly about the O's nose. In the bottom of the chamber is an opening 5 mm. in diameter from which extends downward a tube 3 cm. long. The edges that come in contact with O's face are padded with felt and the whole affair is supported rigidly at a height such that O can quickly and easily adjust his nose in the chamber and thus breathe through the single 'nostril.' The metal parts are nickel-plated so that there is little tendency for the odorous substances to adhere to the surfaces. The volume of the chamber when the nose is in position is so small that one breath exhausts the air in it many times over. The 'nostril' extends downward and away from the O through a cardboard screen which completely hides the stimulus and its mode of presentation from O. Directly beneath the 'nostril' is a track and a slide with two compartments for carrying the stimulus bottles. The track is at such a distance below that when a stimulus bottle in the slide is moved into position its opening is 2 cm. below the opening of the nostril. Between the two is a screen through an opening in which the neck of the bottle must be passed to bring it into the position for stimulation. At other times the opening in the screen can be kept closed and all air from the region of the stimuli excluded from the 'nostril.' The stimulus bottle is brought into the stimulating position by pushing up on a plunger which extends through the track and slide. This raises the bottle until the 'nostril' just enters its neck. The Os are trained to breath rhythmically in time with a metronome at the rate of one inhalation and one exhalation every

two seconds—one second each. At a signal O begins to breath in time with the metronome and to indicate his inspiration and expiration by motioning with his hand, in view of the experimenter, up and down for each respectively. After 2 sec. (one complete respiration) E moves into place and raises into position the first stimulus, holds it there for 1 sec., then, during the next second (expiration) removes it and brings the second into position for stimulation at the beginning of the 3d sec. (second inspiration). This apparatus and procedure insures perfect dirhinic breathing, constant stimulation time, constant interval between paired stimulations, constant intensity for any stimulus, and prevents O from forming any associations with the appearance of the stimulus bottles or with the mechanism of stimulation. When the stimulus bottles are not in actual use in the slide, they are kept tightly corked. After every five presentations —or less if O noted any residual odor—the inside and outside of the chamber and of the 'nostril' were sprayed with H_2O_2 which effectively removed all residual odor without imparting one of its own. The entire apparatus was set up under the hood of a ventilator which prevented the accumulation of odors in the room.

Procedure I

The Os were instructed as follows: "You will be presented with two odors in succession. You are to judge the degree of qualitative similarity between them on a scale of 10. If the two seem identical in quality, your judgment will be 10; if the second seems very much like the first but not quite the same, your judgment may be 9 or 8; if they are partly similar, 6, 5, or 4; if the similarity is very slight, 3, 2, or 1; 0 indicates total dissimilarity between the odors. You are to make your judgments only on the basis of the qualities of the odors. Disregard entirely all other criteria, such as intensity, warmth, cold, sting, taste and the like. The two odors will be presented only once. Do not, however, force a judgment or guess, if you are in doubt. Following your quantitative judgment, you are to describe the basis for your judgment."

Two sets of ten stimuli were presented, every one with every other one in both time orders, including every odor with itself. Four Os made the comparisons; Se and Sc, graduate students, R, an undergraduate, and D, the writer. Two hundred comparisons were made by every O in the course of training prior to the regular experimental sets.

Table II

Variability of Judgments of Olfactory Similarity

Average Mean Variation among Os	1.53
Distribution of Mean Variations, % 0	10
% less than 1	29
% " " 2	70.5
% " " 3	94.5
% " " 4	99.5
Maximum Mean Variation	4.4

The results in Table II indicate that our Os have been able, under our conditions, to attain a fairly high degree of agreement in their judgments after a moderate amount of practice. It is significant that 70 per cent of the M.Vs. are less than 2 on our scale of 10, in spite of the fact that we are unable as yet to control exactly the criteria upon which the several Os base their judgments.

Table III

Stability of Olfactory Stimuli

Average of judgments when every odor was compared with itself	9.74 ± .4
% of judgments of 10 when every odor was compared with itself	85
% of judgments of 10 between other odors	12.5

When, in the course of the haphazard presentations without knowledge, the same odor was given twice as a single comparison pair, the judgment of total similarity was elicited almost invariably. This judgment was not dependent upon the recognition of the second odor as coming from the same stimulus as the first. The two were always perceived as distinct experiences though identical in quality.

TABLE IV

EFFECT OF THE TIME ORDER OF PRESENTATION

Average deviation*	Set I.	Set II.	Total
	2.3 ± 1.6	1.3 ± 1.7	2.1 ± 1.6

The values in Table IV show that the difference in time order of presentation produces less variation than occurs among Os. The distribution of the actual deviations in Fig. 1 gives further indication of the relative stability of the

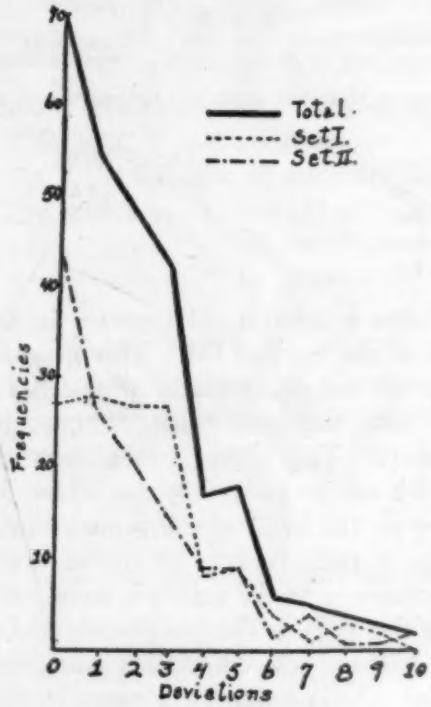

FIG. 1

odor experiences. The improvement from Set I to Set II may depend either upon practice or upon the particular odors employed. Probably both factors contribute. Some olfactory stimuli, then, are more stable than others, but among our 20 stimuli all but one (nicotine) gave very con-

* (These are the averages of the actual deviations and therefore, twice as large as the deviations from a mean.)

sistent results both when the time order was changed and when an odor was compared with itself.

Procedure II

We prepared a set of 10 pairs of odors and presented them under the same conditions and with the same instructions as in Procedure I. Every pair was presented 25 times—12 in one time order and 13 in the other—in haphazard series of 10 presentations.

TABLE V

EFFECT OF TRAINING AND OF STABILIZATION OF THE CRITERIA OF JUDGMENT

(Average of 10 Mean Variations, 250 Judgments from every O)

Av. M.V.		*Variation among Os*	
D.....	1.23	Av. M.V. All Os.....	2.03
Se.....	.95	" " Os D.-Sc.....	.8
Sc.....	1.43	" " Os Se.-R.....	1.0
R.....	1.54		
Av.....	1.29		

Table V shows a substantial increase in the stability of the judgments of the various Os. This means that every O has been able to set up criteria of qualitative olfactory similarity that have remained constant throughout the series of 250 judgments. The same homogeneity does not hold among Os, if we are to judge by the larger M.Vs. All Os are not judging on the basis of the same criteria. However, when we compare the averages of the several Os with one another, it becomes evident that we have only two sets of criteria among the 4 Os. The judgments of D. and Sc. and those of Se. and R. show a remarkably small variation within the pairs of Os. We are justified, then, in concluding that in such cases the two Os are giving comparable judgments.

Our procedure here was designed to bring out stable quantitative judgments. In addition, however, the Os made the attempt to describe their criteria of similarity among odors. In these reports we find several characteristics of odors noted. (1) Some odors have a definite temporal course. (This is not adaptation in the usual sense for the change occurs during a single presentation of 1 sec.) (2) Some odors

persist in a characteristic way during the presentation, totally independent of their intensities. (3) Adjectives such as 'sharp,' 'smooth,' 'flat,' 'clear' occur frequently and seem to be more descriptive than class references.

Procedure III

The 10 pairs of odors that were used in Procedure II were repeated 10 times in succession in alternate orders and the Os were instructed as follows: "You will be presented with pairs of olfactory stimuli as before. After the presentation of every pair you will be asked (1) to make judgments of the qualitative similarity of the 2 odors on the scale of 10; (2) to describe each odor as completely as you can; (3) to point out what aspects, if any, are common to the two odors; (4) to point out what aspects, if any, are dissimilar in the two odors. Try, so far as possible, to give descriptive terms to the olfactory qualities. Avoid associations with odorous substances. Every pair will be presented ten times successively."

TABLE VI

DISTRIBUTION OF DESCRIPTIVE CATEGORIES

	Vol.	Bri.	Tex.	Compl.	Loc.	Anal.	Class	Aff.	Int.	Obj.
D.....	4	7	17	2	3	13	5	2	0	0
Se.....	1	0	5	8	0	16	5	5	1	17
Sc.....	9	1	7	2	1	9	14	5	4	17

(Vol., volume; Bri., brightness; Tex., texture; Compl., complexity; Loc., localization; Anal., analogy; Aff., affective tone; Int., intensity; Obj., object.)

The descriptive terms applied to the several odors fall into the ten categories listed in Table VI. (1) Volume: The odors seemed to vary in size, bulkiness, weight, etc., *e.g.*, "It is thinner . . . and more voluminous" (Sc., Benzine); "The first seemed larger and fuller" (Se., Benzine). (2) Brightness: The odors varied in a brightness-dullness dimension, *e.g.*, "The first is bright and stringy. The second is less bright" (D., Eucalyptus and nutmeg). (3) Texture: The odors possessed a substantive texture or grain, *e.g.*, "The first is rough or grainy in texture, harsher and less compact than the second" (D., Benzine); "The second odor

is soft" (Se., Citral); "It seemed dryer, softer, smoother than the other" (Sc., Meat extract). (4) Complexity: The odors seemed to have several separable parts, *e.g.*, "The first had more qualities" (Sc., Benzine); "There seemed to be a temporal qualitative change in these odors" (D., Toluol and Bay oil). (5) Localization: The odors were variously localized in the head, *e.g.*, "The first odor seemed to be localized in the center of the nose, the second on the sides of the nostrils" (D., Benzine and Fish soap); "The first one, the harder to smell, seemed to 'stick' in the nostrils and to be localized farther out, as it came into the nose; the second seemed to come all the way up in and to spread out brightly" (D., Toluol, Bay oil). (6) Analogy: Analogical references were made to other sense departments, *e.g.*, "The first resembled sweet; the second was more like salt" (D., Toluol, Bay oil). "A sweetness was common to both odors" (Se., Origanum, Turpentine). (7) Class: The odors were referred to a class of olfactory stimuli, *e.g.*, "The second odor was spicy and burned" (Sc., Cod Liver oil). "The second odor was spicy" (Se., Nutmeg). (8) Affective Tone: "The first odor was somewhat unpleasant" (Se., Eucalyptus); "The second odor was pleasant" (Sc., Nutmeg). (9) Intensity: "The first odor was a dull heavy spice more intense than the first" (Se., Clove, Citral). (10) Object: The odors were identified with or referred to an object, *e.g.*, "The second was an odor of turpentine as in varnish" (Se., Turpentine); "The first odor was like mustard, the second seemed to be complex" (Sc., Pine Needles ex., Camphor).

Of the ten categories into which the characterizations of the several odors fall, the first five may be taken as essentially descriptive in as much as they refer directly to the fundamental nature (qualitative) of the odors. Analogical references, and references to classes of odors and to objects certainly are not descriptive but constitute, rather, evasions of the descriptive task set to the Os. They are, however, the most usual terms applied to odors by our Os and are used especially in other investigations of olfactory qualities. Our Os have, on the other hand, been able to apply truly

descriptive terms in addition to the cruder identifications. The first three of these, volume, brightness, and texture, seem to us to be especially significant. It is possible, then, to describe odors in terms which indicate the nature of the odor quality itself and which bear no reference to the stimuli.

Conclusions

Our experimental results show that Henning's olfactory classes and the accepted color classes are not comparable as he states. While Os will, when properly instructed, arrange odors in series it is evident that the criteria for such arrangements vary from time to time and from O to O. It seems obvious from these facts that the classifications and serial arrangements are based upon variable secondary criteria and not upon the fundamental characteristics of the olfactory qualities. Further, it seems evident that the instability of the results that have been obtained heretofore is not due either to the intrinsic difficulty of judging olfactory qualities or to any peculiar instability of the olfactory stimuli. When the observational task was sufficiently clean-cut and when the presentation of the stimuli was adequately controlled, our Os after a reasonable amount of practice were able to attain a fairly high degree of stability of judgment. The inconclusiveness of the previous work on olfactory qualities seems to be due to the task which has been set for the Os which has permitted all sorts of variable and fundamentally inadequate secondary criteria, to the insufficient control of the presentation of the stimuli, and to the non-descriptive nature of the olfactory categories with which most of the Os have worked. With the determination of, and the restriction of Os to, specific criteria, we may expect further progress and greater agreement in the classification of olfactory qualities.

PRIMARY EMOTIONS

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I. Problem

What does the average teacher of psychology mean when he glibly rattles off the words 'fear,' 'rage,' 'anger,' and 'sex-emotion'? Almost any literary light of the Victorian era, if asked to define these words, would have answered, readily enough: "They are names for emotions possessing distinctive conscious qualities, experienced by everybody, every day. Those easily recognized, primitive emotions constitute the very backbone of literature." I submit that the backbone of literature has been transplanted intact into psychology; that it has proved pitifully inadequate; and that the whole structure of our recently christened 'science' remains spineless in its logical organization, in consequence. Most teachers of psychology, it would seem, are still unable to define these time-worn emotional terms with greater exactness or scientific meaning than that employed by literary men of the last century.

Nor can the average teacher of psychology be blamed. Theorists and researchers upon whom the teacher must depend for his scientific concepts have written many hundreds of thousands of words on the subject of emotions, without attempting definite, psycho-neural description of a single basic, or primary emotion. On the other hand, nearly all writers seem to accept the old, undefined literary *names* of various 'emotions' without question; each writer then giving these terms such connotation as they may happen to hold for him, individually. Consider, for example, the term 'fear.' This word seems to find its way, unquestioned, into nearly every emotions research reported to the literature of psychology and physiology. What does it mean? The

James-Langeites say 'fear' is a complex of sensations, perhaps largely visceral, perhaps not; perhaps the same in all subjects, but probably differing importantly in different individuals. Surely, the unfortunate teacher of psychology can extract little comfort from such vague guess-work. Besides, the physiologists have proved, with their customary thoroughness, that the condition of consciousness traditionally termed 'fear' in popular and literary parlance cannot be composed, characteristically, of sensory content (1).

What, then, of the physiologists? They use the term 'fear,' it appears, quite as blithely and trustfully as do the James-Langeites. Cannon uses the word 'fear' throughout the entire course of his extremely valuable work entitled, 'Bodily Changes in Pain, Hunger, Fear, and Rage' (2). But how does he differentiate it from 'rage,' or from 'pain'? He points out physiological similarities, but no measurable differences between these 'major' emotions. Cannon assumes that the so-called sympathetic division of the autonomic nervous system is always activated by the 'fear' pattern. But he cites various other effects of 'fear,' such as nausea, weakness, vomiting, etc., which would be ascribed, by many writers, to vagus impulses. Moreover, 'rage,' 'pain,' and other 'major emotions' also discharge characteristically into the sympathetic, as Cannon himself emphasizes (2). So we are left, again, high and dry in our search for any specific meaning for the famous word 'fear.'

Some behavioristic psychologists have insisted, commendably enough, that all emotional responses be defined in terms of specific description of stimulus and response. One or two writers have somewhat naïvely assumed that the response of bodily retreat, in various forms, was sufficient to characterize 'fear.' I have elsewhere suggested (3) that such a blanket description of 'fear' is wholly inadequate, since retreat may be the end product of radically divergent psychoneural sets, although retreat seems to be the sort of thing most writers are trying to get at in attempted definitions of 'fear.' Other writers, recently, failing to obtain any adequate description of bodily symptoms of 'fear' (and other emotional

responses), have assumed with still greater naïveté that no emotion can ever be described in terms of a distinctive motor end-effect upon the body, this conclusion being based entirely upon rediscovery of the well-known fact that visceral accompaniments of many emotions are identical, and upon the failure of a given writer's experimental technique to discover other differential criteria. The alternative 'behavioristic' suggestion offered, in at least one instance, is to define emotion in terms of the stimulus situation. This proposition contemplates arbitrarily devising an infinite number of experimental situations to be kept mechanically constant for all subjects, measuring the bodily changes during each, and defining the emotions manifested in terms of the situation imposed. This method, I should think, may not properly be termed behavioristic, but rather the nightmare of a mental tester. For, in my own research experience at least, emotional stimulus situations kept physically constant could seldom be expected to produce results even roughly similar with any two subjects. Thus our emotional definitions must immediately resolve themselves into terms of median, mode, percentiles, and other statistical criteria of individual differences. Must the psycho-physiology of emotion come to this? Must our teachers of psychology, unable at this time to offer more than a literary definition of 'fear,' look forward to the day when they may declaim to eager students, "Fear is that psycho-galvanic-blood-pressure-breathing-ratio score falling within the middle fifty per cent of the scores made by any given group of subjects exposed to the firegong-electric punishments test, known as Shock U (copies at the bookstore, 7 cents each)"?

Obtaining clear-cut types of emotional behavior in adults under experimental conditions is vastly difficult, to be sure. Normal adult human beings have developed great complexity of emotional life, and the conditionings of these complex emotional responses differ widely in different individuals. Standardizing the physical stimulus conditions, however, seems to be a method of deliberately mixing all the subject's emotions in one great, mechanical vat, from which only statistical computations can ever emerge.

A casual examination of differences in type between emotional reactions of adult human subjects to an identical physical stimulus shows clearly how 'general emotionality' is mixed, every day, in the laboratory kitchen. A kiss, for instance, may arouse aggressive response from one subject, escape reaction from another, and passionate love from a third. A rattlesnake (which we used in a series of as yet unpublished experiments) may evoke retreat responses, annoyed indifference, or it may bring forth the exclamation (from girl subject): "Isn't he cute!", followed by symptoms of eager interest. Crude sex pictures, naïvely characterized by certain emotions investigators as 'erotic stimuli,' leave girls and other subjects of highly developed love emotion almost completely cold and responseless, while they produce a sort of general, animal-like, bodily excitement reaction in many males. The bodily effects of all types of emotional stimuli, moreover, are always greatly changed and diminished, frequently disappearing altogether, if the experimenter fails to put himself and the experimental conditions completely 'en rapport' with the subject, protective resistance to control of his emotions by outsiders having become, necessarily, the most habitual of all motor attitudes in the normal human adult. Again, if the experimenter tries forcefully to break down this self-protective resistance to emotional stimulation by the frequently employed 'emotional' shock method, whether the shock take the form of physical punishment or of overintense unpleasantness of sight or sound, little or no emotion results, as far as I can discover, during persistence of the shock effect. The true emotional responses, when they do begin to appear again, are sufficiently jumbled and indeterminate to suit the purposes of an investigator possessed of the most rabid determination to prove emotional analysis into basic response classifications impossible. This effect of emotional inhibition and confusion following shock is often especially noticeable in female subjects, as well as in children (especially mentally deficient children).

To put it another way, standardization of physical stimulus situation, in emotions research, fails completely to

standardize the receptive mechanism of the subject to which the stimulus is applied. No matter whether one subscribes to the James-Lange theory, to the Watsonian doctrines, or to a theory of motor-consciousness such as my own (4), it must be agreed that the differential factor in emotion is not arrived at until after the original sensory impulses elicited by the emotional stimulus succeed in effecting relationship, somewhere in the central nervous system, with a distinctive pattern of motor discharge paths. No possible combination of sensations of origin can, according to any published theory of emotion, compose the peculiar character of the emotion in question. We know that certain sensations may be connected with one motor pattern in subject A, and that the same sensations may be connected with a widely different set of motor discharge paths in subject B. If, then, the motor pattern L is the mechanism we wish to reach and stimulate, it becomes obviously necessary to elicit whatever sensation group may be connected with L in the subject we are examining. Only the most superficial thinker will be dismayed by the fact that we find ourselves compelled to stimulate widely divergent sensory receptors in order to evoke comparable emotional responses in different experimental subjects. Emotions are connector-motor phenomena, each basic emotional mechanism probably being connected with nearly all the sensory end-organs of the body. *The true receptors for emotions which must be identically stimulated to produce comparable results lie in the integrative nervous mechanisms*, not in the sense organs at the surface of the body. And to stimulate the same sense organs, therefore, in different subjects gives no assurance that similar emotional receptor mechanisms have been stimulated. If an experimenter, working upon a problem of sensory response, stimulated with blue light the tympanic membrane and the retina, should we marvel at the resulting diversity of reaction? And should we, furthermore, expect him to conclude that there exist no discrete sensory mechanisms, but only sensationality?

Three points of experimental technique, peculiar to emotions research, seem to me essential if measurements of

bodily changes resulting from the stimulation imposed upon adult subjects are to be attributed to emotional response. (1) Some preliminary analysis of the subject's emotional organization and conditioning should be made. If a given investigator possesses unusual insight into the personalities of others, such analysis may be made rather quickly, and, in most cases, without serious difficulty. Such insight is a practically demonstrable ability, and does not in the least depend upon the experimenter's own idea of himself. If a would-be emotions investigator is possessed of a paramount desire to prove that the emotions of others are non-existent, or are of negligible importance in the subject's behavior, preliminary analysis becomes virtually impossible, and the contemplated research should not be undertaken, since its results are foredoomed to be not only negative, but grossly misleading. In general, emotions measurements made upon adult subjects whose cases have been studied for a preliminary period of from one to five years, with a view to discovering the main trend of emotional organization and conditionings, may be expected to yield fairly reliable results. The psychoanalysts, who have performed a major portion of the applied work in emotions to date, have long realized the necessity of such a comparatively long period of preliminary analysis.

(2) When the time for definitive experiment arrives, individual stimulus situations must be devised, to a considerable extent, for each subject. Adaptations of one situation may frequently be made for all; and this procedure is preferable, of course, when possible.

(3) Throughout experimentation, the successful emotions investigator must be able—and willing—to put himself completely 'en rapport' with each subject, individually, maintaining this sympathetic relationship despite the subject's varying moods, and even when an emotional stimulus situation must be made unpleasant. If any subject is permitted to assume either a self-protectively reticent, or an openly antagonistic attitude toward the experimenter, emotional responses will immediately tend to become inhibited, altered, and confused. Since neither the subject himself, nor the

experimental apparatus at present available, is capable of analyzing out such conflicting conditions accurately, the only proper course is abandonment of the experiment at this point. Unless the subject can be brought to identify the experimenter to a considerable extent with himself, all deeper and more intimate emotional responses can, and will be altered, in my experience.

Nor is the technique of establishing rapport objectionable on the ground that it is only a subjective procedure, free from objective control. Substantial failure of bodily response to appear when the stimulus is of a type known to be normally effective may be used objectively to indicate failure of rapport. Here, again, a preliminary study of the individual subject proves invaluable. By its aid I have found that a subject may successfully resist manifestation of emotional response in the presence of an experimenter not fully trusted, even when tested by such a sensitive instrument as the galvanometer, though expressing that same emotion freely when possessing complete confidence in the examiner. It is quite obvious to the average subject that the 'impersonal,' 'cold,' or 'smart-Aleck' types of emotions experimenters intend to use the data they obtain for their own purposes, regardless of the subject's interests and feelings. Thus most male subjects will withhold all crucial reactions in love experiments conducted by other males, and they will withhold much important information from sex questionnaires, under similar conditions. If examined by a woman experimenter, however, who is felt as a person wholly sympathetic and unwilling to hurt them, these love inhibitions are greatly reduced or may frequently disappear altogether. If a male subject is less love-developed, a male experimenter with unusual friendliness and personal understanding may prove successful in establishing the necessary rapport for positive experimental results. In love experimentation under conditions freed from conventional limitations, easily observable objective check of removal of inhibitions due to the existence of adequate rapport is always available, with subjects of either sex.

If the foregoing suggestions possess any weight in de-

termining experimental technique for emotions investigation, we are confronted next with the problem of postulating tentative basic emotional response patterns in terms of which preliminary analysis of any subject's emotional responses may be made. Initially proposed types of response only constitute working tools, to be further described and defined by experiment. Like the 'guessed average' in statistical methods, the fact that these tentative emotional primaries may not conform closely to our final result does not in the least militate against their temporary value as media for definitive calculation. If a very bad guess is made at the beginning, the process of correction may prove longer, and more cumbersome; but the selection of a tentative unit makes possible a finally correct result, none the less surely. With workable, 'guessed' primaries at hand, emotions investigators all over the world might unite to good advantage in examining and correcting the initial guesses.

With 'guessed primaries' available, the experimental technique above suggested might be applied as follows. (1) Preliminary analysis of development and conditioning of subject's primary emotional responses could be made by competent investigators, in terms of the tentative, basic response classifications. (2) Stimulus situations appropriate for laboratory use might be devised, which would make sure of actually reaching and stimulating each subject's primary emotional responses. (3) Competent investigators might carefully adjust their relationships to the subjects to be examined over an adequate period of time to insure necessary rapport (following somewhat the present psychoanalytical procedure, but avoiding personal love relationships which may prove fully as complicating a factor in proper emotions research as personal antagonism or resistance).

The problem, then, which this paper is intended to present, may be stated as the *formulation of tentative, basic, emotional response types defined in sufficiently specific terms to furnish workable hypotheses for experimental determination of true primary emotions.*

II. Method of Formulating Emotional Primaries

In 1923 I tried to prepare for publication a number of still unpublished researches upon bodily symptoms of emotion. I found myself utterly unable to interpret the results with even a passing show of meaning. Two main difficulties appeared. First, the literary names for emotional responses, 'fear,' 'anger,' 'sex-emotion,' and others, seemed to me clearly complex, overlapping, and based upon wholly undefined neural mechanisms. Secondly, the visceral changes symptomatic of obviously different emotions were so completely overlapping in my results (as Cannon showed they must be) that only the qualitative and relatively quantitative aspects of resulting measurements (such as the form of blood-pressure curves and relative amounts of variation) appeared to possess the slightest analytical significance. Such qualitative and relative criteria seemed to me too vague to be of substantial scientific value. There remained the option, on the one hand, of giving up all attempts at scientifically descriptive treatment of emotions data, and resorting to purely statistical (and hence individually meaningless) speculation; or, on the other hand, of attempting to supplement the data at hand by study of neurological material, and by sufficient behavioristic observation, of the Watsonian variety, to formulate tentative emotional response primaries. I chose the latter course, and this paper reports the preliminary results of the last three years' work on this problem.

III. Basic Types of Emotional Response

Observations of very young children have been reported by J. B. Watson and R. R. Watson (**5**), and by Mary C. Jones (**6**). Watson has made a careful analysis of these results, arriving at the conclusion that there exist three primary emotional response types, the mechanisms for which are inherent in the organism at birth (**7**). Watson names these three emotions —rather unfortunately, as he himself admits—'rage,' 'fear,' and 'love,' cautioning the reader, at the same time, to strip these time-worn terms 'of all their old connotations.' Were

this psychologically possible, after the words have become fixed in our language habits with varying meanings during a lifetime of use, there might be little necessity for devising new terms. In fact, the three terms just mentioned may well be retained as names of complex emotions, once the primary elements in these complex responses have been isolated and defined. But our basic emotional response elements must receive names free from the heritage of literary confusion, if they are to serve their purpose.

1. *Dominance*

Watson describes the stimulus situation that gives rise to unlearned 'rage' response simply as 'hampering of bodily movement.' The emotional response, in briefest summary, consists of easily observable increase of motor energy, evidently released from the infant's central nervous system into motor nerves innervating large groups of skeletal muscles designed to resist or control environment. This release of additional energy was not, apparently, in the child responses studied by Watson, selectively directed toward those muscles capable of acting upon the hampering stimulus, but rather seems to have consisted of additional motor energy, discharged indiscriminately into all effector organs receiving constant tonic energization.

Here we find a clearly marked type of integrative phenomenon. Sherrington has shown (**8**) that the postural, or tonic nerve impulses which hold the body upright and in a condition for efficient use against the forces of gravitation, atmospheric pressure, and other environmental influences continually acting to hamper the body's movements, may, to a considerable extent, originate in proprioceptors which respond to tension in the muscles themselves, no matter whether this tension is actively or passively imposed. That is, we have forces of the environment continually pulling and pushing our bodies. These pulls and pushes, acting upon the muscles of the body constructed to oppose them, tense these muscles mechanically. Afferent impulses are thereupon set up in the muscular proprioceptors, which result in reflex

motor impulses returning to the muscles in question. Thus, the constant contraction of these muscles is reflexly regulated to counteract gravitation and other opposed forces. This continuous activity of postural, or tonic reflexes, then, must form a constant background of motor discharge, even in the newborn infant, and with this tonic discharge most phasic or intermittent reflexes must form relationships of alliance or antagonism.

When the movements of an infant subject are hampered, the particular muscles affected are mechanically tensed beyond their natural, or tonic condition. Forbes, Campbell, and Williams have shown (9) that increased tension of the muscles, no matter whether actively or passively brought about, results in immediate increase of reflex motor discharge calculated further to contract the muscle, and oppose the hampering stimulus. Thus, the effect of hampering any voluntary movement is to elicit greater muscular effort to oppose the obstacle, and carry out the original movement. Although the simplest mechanism of reënforcement, as described, consists of specific augmenting reflexes, set up by increased tension in the muscles which are hampered, there must be a marked central spread of this opposition energy released against the enemy, following the usual laws of central induction and overflow. The total energy of opposition to environment, released in response to an opposition stimulus, may be evidenced in the infant behavior described by Watson. It also seems at least possible that there is a breakdown element in the infant 'rage' responses, which, in ordinary terminology, might be described as a thwarting, or baffling of the specific reënforcement mechanism. Perhaps such partial breakdown of the specific reënforcement mechanism may be evidenced in the overextensity of the central overflow, involving nearly all parts of the body simultaneously, instead of being limited to those parts more intimately connected with the particular muscles mechanically tensed by the stimulus.

If the infant response does contain this baffled, or thwarted element, then it may be said to contain two distinct

emotional response units: first, a considerable volume of reënforcement energy, allied with constantly discharging tonic reflexes, all directed toward overcoming an opposition stimulus; and, second, a certain central thwarting, or breakdown of this reënforcement, possibly resulting from central release of more intense motor energy than available efferent nerve trunks can carry. The emotional response complex consisting of these two elements may be termed *rage*, just as Watson terms it, without danger of undue confusion with the old literary terminology. This leaves unnamed the basic type of reënforcement response, specifically directed toward counteracting the hampering stimulus, and thus carrying out the original movement. Since the obvious purpose of this motor reënforcement is to overcome, or to *dominate environmental opposition*, I suggest the term *domination response* for the entire mass of reënforcement reflexes set off by an opposition stimulus. This basic type of response, when referred to as a primary emotion, may conveniently be called *dominance*. The term 'dominance' seems fairly descriptive of what actually occurs, and does not have the disadvantage of conflicting with previous psychological usages of the same term.

Without elaborating upon my own results at this time, I may add something to the emotional meaning of the term 'dominance' by briefly summarizing its basic rôle in human behavior according to my own observations.

From 1913 to 1920 a good share of my time was devoted to work upon systolic blood-pressure, reaction time, and other bodily measurements of deception. In my initial report on the systolic b.p. deception test (**10**), I concluded that "a significant lying curve is a function of the struggle between involuntary impulses to express fear in response to awareness of danger, and the voluntary focussing of attention to exclude the fear from consciousness." It might have been more accurate to describe the voluntary part of the response as an *effort to dominate the opposition stimuli presented by the questions of the cross-examiner.*

Following the accepted usage of the word 'fear,' I assumed,

at that time, that it was the fear element that sent the systolic blood-pressure up. I began to perceive the error of this interpretation when we found distinctly *negative* reaction time symptoms of deception; that is, subjects who could lie faster and more efficiently than they could tell the truth. It became at once apparent that more central nervous energy was being involuntarily released during the efforts to deceive, and that this additional energy was evidenced in the shortening of reaction times *of those subjects who showed least 'fear.'* Apparently, then, 'fear' operated to inhibit the extra deception energy, while the *dominant effort to defeat the examiner must be the factor that raised b.p. and shortened reaction times.*

Goldstein later published results (11) showing that a still greater proportion of her subjects proved of negative reaction time type during deception, when no more mental work was required during deception than during truth-telling. In a subsequent unpublished experiment, E. H. Marston and the writer measured association reaction times of subjects of both sexes, allowing the subjects time to think up any association word they chose, after hearing the stimulus word and before being called upon to respond with the word they had selected. (Blood-pressure was simultaneously recorded with Tycos sphygmomanometer.) Comparisons of reaction times when the subject was given the 'true' word (printed on a list given to the subject) and reaction times when he was attempting to deceive the experimenter by substituting a word of his own were then made. The results of this experiment finally convinced me that the *active emotional response element characterizing deception is not 'fear,' but rather dominance.* As definitely described above, the 'fear' element is undoubtedly the inhibitory one, acting to lengthen reaction times, and to reduce systolic blood-pressure, whenever present in sufficient volume.

Examples of domination response in every-day behavior are not far to seek. If one tries to take a favorite toy away from a boy five years old, dominance in its purest form normally appears. The child's sudden increase of energy in

seeking to recover his plaything is frequently startling, and the persistence of his demand over a period of hours, or even days is remarkable. Equally pure dominance may appear in the behavior of the boy's father upon suffering defeat at golf, an activity to which no economic necessity drives him. I knew of one case where a professional man, thus defeated by a friend, got up at 5 A.M. every week-day morning for an entire summer in order to out-practice his opponent and defeat him in a club tournament at the close of the season. In this instance, a really huge amount of additional energy was released to overwhelm the hampering or domination stimulus. This seems to be nothing more nor less than the simple, basic, emotional response of dominance, described at the beginning of this section. In my own clinical work on emotions, carried on since 1923, I have found dominance to be the mainspring of behavior in nearly every male subject whom I have had the occasion to observe.

In summary, *dominance may be defined as central release of additional motor energy directed toward dominating obstacles to a reaction already in progress. The adequate stimulus to the domination response may be defined as any stimulus tending to dominate a reaction, or series of reactions, already in progress.*

2. *Compliance*

Watson concludes that loud sounds, and loss of bodily support evoke an unlearned 'fear' response from human infants. The typical reactions included in a 'fear' response are crying, 'jump' or start of the whole body, unchecked visceral responses such as urination, etc., and later, when the child is older, crawling away from the stimulus as rapidly as possible. Summary of these reactions might be made, it seems, under the head of compulsory giving up of responses already in progress at the moment of stimulation, and a somewhat unsuccessful attempt to respond appropriately to the 'fear' stimulus, whatever its nature may be. Here again, the complicating response element of a breakdown factor, in the nature of an inhibition of the attempted intercurrent response, may be evidenced. That is, the child is compelled

to give up previous reactions, and there is present an evident tendency toward making the appropriate response to place the organism in adjustment to the new stimulus; but the intensity of the loud sound or unmitigated pull of gravity appears partially to inhibit that very tonic motor energy (considered above), which must be used in compliance with the intercurrent response, to bring adjustment about.

By way of bringing out the basic response element here involved, without the breakdown factor, let us consider the native grasping and reaching reflexes, also described by the Watsons (5). In these instances, the child's previous reactions are altered, and in some instances supplanted altogether by the intercurrent response, as soon as the stimulation of finger or rod against the child's palm is made sufficiently intense, or as soon as the intensity of the visual stimulus of an object moving toward the child becomes strong enough to control the appropriate reflex paths. The child's mother is apt to describe this process by saying that the infant 'takes an interest in' his rattle, toy, or mother's hand; and with adult subjects many psychologists would refer to the matter in the same way. There exists, moreover, ample justification for the classification of 'taking an interest' in objects as a basic emotional response. Loss of 'interest' in environment is listed among the affective irregularities of several major psychoses, and when this symptom is far advanced, as in psychopathic states of withdrawal from environment, emotional control of the patient by his physicians is virtually lost. From my own studies, I have reached the conclusion that this taking an interest in environmental objects or, as we may term it, *compliance* response, ranks with *dominance* as a basic emotional element. *Compliance*, in this sense, *means control* (but *not* inhibition) *of tonic motor discharge reënforcement by a phasic reflex.*

Forbes, Campbell, and Williams (9) found that, in case of active contraction of a muscle in response to a single stimulus, the afferent impulses initiating additional motor discharge, of tonic type, into that same muscle, appeared in several distinct groups. The first of these groups seemed to be

initiated when the muscle began to contract while later groups appeared to depend upon tension developed in the muscle by whatever environmental resistance it met in the course of its contraction. This finding seems to offer the suggestion of distinctly separate mechanisms for central release of extra tonic energy to selected muscles. *One increment of additional energy may be centrally released and discharged into a given muscle whenever that muscle responds to a phasic reflex.* And a second series of increments may be subsequently discharged into that same muscle, if the muscle be dominated by a resistant environment. The first *may be considered as the mild, though extremely important emotional response element of 'taking an interest in' the stimulus, and may be termed compliance.*

The second we have already considered at length, and have termed 'dominance.' It may also be noted that just as a dominant stimulus brings forth a dominant response, so a compliant stimulus is required to bring forth a simple response of compliance. For if a phasic reflex fails to comply with the motor organization and set of the organism, it cannot succeed in contracting the muscle in the manner necessary to stimulate adequately its proprioceptors, and set up the supplementary, or compliant reflexes.

Compliance, as a basic type of emotional response, is not to be confused with inaction, or inhibition, though inhibition of the reaction previously in progress may be made necessary by compliance with the new stimulus. The inhibition making possible a response of compliance with a new stimulus may be compelled by the overintensity of the new stimulus itself. But after this inhibition has occurred, no compliant response with the new stimulus can be evoked unless the new stimulus is sufficiently *compliant* with the mechanisms of the organism to initiate a reflex of its own, contracting a given muscle or group of muscles. Certain lethal gases, for instance, may destroy life itself without compelling the least compliance from the organism destroyed. On the other hand, a pin prick, a drop of perfume, or a red apple on a high bough may evoke prolonged and energetic responses of compliance.

"You can lead a horse to water but you cannot make him drink" is true because, in leading him to water, you are able to comply with the mechanisms of his organism, arousing the reflexes necessary to produce the desired result; whereas, in trying to make the animal drink by forceful means, you are not complying sufficiently with his nervous system to evoke the drinking reaction. If one were able to dry up the horse's œsophagus, or treat other bodily tissues in appropriate ways, the old proverb would have to be abandoned.

The most frequent result of compulsory compliance seems to be that the stimulus overreaches itself, as it were, interfering with the very reflexes that constitute compliant response by partially inhibiting them, along with the initial inhibition of previously existing reactions. The 'fear' behavior of the children studied by Watson perhaps represents this type of situation. Previous reactions are initially inhibited by the overintense stimuli imposed. Then occurs a 'jump or start of the whole body.' May this not represent an abortive attempt of the infant organism to react vigorously to the new stimulus, only to have the new reactions nipped in the bud by the very intensity of stimulus which made them possible by inhibiting their predecessors? As the children became older, in fact, Watson notes that they began to crawl rapidly away from the unpleasant stimulus, thus indicating compliance with the stimulus as soon as its overwhelming intensity could be endured. Following this analysis, the conclusion suggests itself that *'fear' response*, as found in the behavior described, *is a complex emotional response, containing two elements: simple compliance, and partial inhibition, or breakdown of that compliance.* The term fear, thus defined, may be used, it would seem, with definite meaning, and without undue confusion with previous literary usages of the word.

When *dominance* also enters the picture as a reënforcement of the compliant reaction, we have a still further complication of emotional response elements. Psychologists have been running away from James's bear for many years. I have already suggested, elsewhere (3), that no fear need be felt by

bear-chased fugitives who are sufficiently fleet of foot. The fear seems to be caused, in some way, by perception of the fact that the subject is not running away fast enough. The bear, in this case, is a conditioned stimulus, sufficiently intense to inhibit existing reactions and capable of evoking reflexes which discharge into appropriate running-away muscles. The initial tensing of these muscles, by the bear reflex, is followed by proprioceptive stimuli evoking a compliance response, which sets the leg muscles in question to running away with adequate vigor. Were this the full extent of the emotional responses involved, we should expect to find the running psychologist experiencing only a pleasant interest in his flight from the bear, as in his morning exercise.

But there follows a certain amount of complication. Our friend perceives that he is not running away fast enough, and that the bear is about to catch him. This effects a cumulative increase in the intensity of the bear stimulus, in exact proportion as the ferocious animal approaches his prey. As the intensity of stimulation increases, this very intensity becomes an opposition stimulus, interfering with, and threatening to inhibit altogether, the compliance response of running away. The bear, in other words, has unwittingly complied with his intended victim to the extent of stimulating him to the compliance response of flight. Now the animal is bent upon ending that compliance response altogether. In this new aspect, the bear represents a dominant stimulus, hampering the compliant movements of the runner. Dominance must combat dominance. If a sufficient intensity of dominance response is evoked in the fugitive, the increasing intensity of stimulation from the approaching bear may be successfully combated; the compliance response of running away will be saved from inhibition; and the addition of newly released dominant energy to the running reactions may enable the psychologist finally to escape from his traditional enemy.

But James's original illustration would not have it so. James said fear was felt. So we must suppose that the runner's dominance response, released under the handicap of

being controlled by compliance, fails to prevent partial inhibition of compliance reaction. In which case, we have precisely the same picture of *compliance response in conflict with inhibition at the hands of that overintense stimulus which initially evoked it* that appeared in the cases of children's fears of loud sounds discussed by Watson. This is true fear, as above defined; and the only rôle played by the complicating dominance response is that of unsuccessful ally to compliance.

In the deception test situation, I believe we have simply another instance of the bear drama so frequently enacted. The cross-examiner is the bear. The liar takes the place of the running psychologist. The victim tries to comply with his enemy by answering all questions quickly, and without falling into the wayside pitfalls of logical inconsistencies, endeavoring to increase all the while the distance between himself and his pursuer. Knowing the real truth of the matter in issue, the deceiver perceives each crucial question as a closer approach of the bear. Such closer approach constitutes, as we have seen, a dominant stimulus threatening the compliance response of flight with total inhibition. The liar reacts, like the fugitive, with a dominance response intended to prevent the breakdown of his compliance. It is this dominance reaction, as above noted, that sends up the blood pressure, giving the typical shaped deception curve found in many instances. If the subject is a successful liar, he is able to command sufficient dominant energy to make good his deception. This unusually powerful dominance response clearly reveals itself in an unusually high, though well-controlled, systolic blood-pressure curve. If his reaction times are being measured, this successful liar cannot prevent the additional energy which he is compelled to use from appearing, also, in the form of shortened reaction times, and he consequently turns out to be a fairly consistent negative type reaction time subject. If, on the other hand, the liar's dominance proves insufficient to meet the increased intensity of stimulation from his examiner's cross questioning, inhibitions in his responses begin to appear. The blood-pressure curve in such cases tends to be more variable and irregular,

the deceiver's crucial reaction times are clearly lengthened (as all deceptive reaction times should be according to arm-chair manufactured definitions of 'deceptive consciousness'), and contradictions and inconsistencies—leading often to confessions in criminal cases—usually creep into the alibi or other false story.

The deceptive emotional response complex consists of dominance contending against an admittedly stronger antagonist to carry out a basic response of compliance with that antagonist; or to put it more concisely, *deceptive emotion is dominance contending against fear.*

3. *Submission*

The love mechanisms are, as Watson points out, much harder to study than are the appetitive responses because of the anti-love conventions of Western civilization. To treat the subject in the naïve and honest manner of the original Arabian Nights stories is very likely to be misunderstood. It seems also to be the fact that neurological studies have largely been confined to considerations of the negative factors. Thus, Martin and Tainter (12) have shown that original inhibition of erection of the external genitals starts in the brain, and travels down the cord and pelvic nerves; which in itself furnishes adequate description of the mechanism by which social stimuli are able continually to suppress and modify natural love behavior. It seems, however, that this conventional difficulty would cease to have weight if the elements of love behavior could be shown to consist of simple, integrative relationships between various groups of tonic and phasic motor impulses. Of these relationships, reactions between the sexes would furnish but one example.

Watson found that stroking of the skin, tickling, patting, and gentle rocking brought forth infant responses which he aptly termed 'love' reactions, using the term 'love,' as he says, 'in a much broader sense than it is popularly used.' The responses brought forth, however, seem to me clearly analyzable into two classes: reactions of *submission*, or voluntary yielding to whatever stimuli may be imposed;

and responses of *inducement*, or active solicitation of attention and stimulation from the experimenter. By far the largest number of reactions mentioned by Watson fall into the submission group. Cessation of crying, smiling, erection of penis, and even gurgling and cooing, all seem to result from mere removal of the customary tonic inhibition placed upon such reflexes, with subsequent evoking of mild, pervasive reactions, appropriate to the intercurrent stimulus, apparently devoid of tonic reënforcement of either compliant or dominant type. The outstanding characteristic of all groups of motor impulses thus submissively permitted to enjoy unimpeded motor discharge seems to be low intensity.

If we assume that the cessation of crying, for instance, is brought about by inhibition of motor impulses of dominant or compliant type resulting from stimuli antagonistic to the tonic set of the organism, then we find the first peculiarity of the submissive response to be that its intercurrent impulses of low intensity manage to inhibit, or supplant existing opposition impulses of much greater intensity. Is this phenomenon attributable to the overwhelming volume of the intercurrent motor discharge, or to an inherent prepotency of its reflex paths? Attempted answer to this question would be, so far as I know, sheer speculation.

The second peculiarity of submissive motor discharge seems to be that after supplanting dominant responses it effects marked diminution in the intensity of the tonic motor discharge itself. That this diminution of tonic innervation may occur in sufficient degree to alter substantially the reflex equilibrium of the organism is strongly indicated by very extensive drops in systolic blood-pressure found occasionally at the height of love excitement in female subjects.

In light of these and other facts, I should be inclined to accept submission emotion as a unitary, basic type of emotional response. Any stimulus capable of evoking this type of reaction may be considered as itself submissive to the subject organism in that it is *less intense* than the organism's current energy discharge. It does not seem to overwhelm, or dominate the subject organism by force, but rather brings

about a spontaneous lessening of the subject's resistance to it, until the subject has become *less strong* than the stimulus. Although the exact mechanism of this self-weakening process is unknown, a significant guess at its nature may be made on the basis of our knowledge that tonic motor discharge is at least partially a resultant of proprioceptive stimuli set up by the tensing of muscles tonically innervated. Any stimulus which has the effect of easing the tension placed upon a given group of muscles by existing opposition stimuli must, by just so much, diminish the total stimulus to tonic discharge, and consequently must diminish the tonic discharge itself.

How may such a reduction of existing tension be brought about? If the submissive stimulus itself evokes reactions wholly in accordance with the subject's resting equilibrium, and if it supplants responses to stimuli which have been opposing this equilibrium (as in the infants' behavior now under analysis, where reactions to stroking and patting supplanted crying responses), then such a reduction of muscular tension will inevitably be effected in direct proportion as the subject responds to the new, or submissive stimulus. Secondarily, as tonic motor discharge is reduced, intra-muscular tension must suffer a further tonic diminution. Thus a circular, cumulative mechanism must be set in operation, tending to produce just such a spontaneous, automatic reduction of tonic energization as we have seen, in fact, occurs during that behavior which has been termed true submission response.

In so far, then, as a given stimulus is so much weaker than the subject that it cannot resist the subject's action upon it, and in so far as it evokes actions allied with the subject's natural reflex equilibrium, it may be termed an adequate submissive stimulus. The cumulative weakening of existent motor energy to or below the level of impulse intensity evoked by such a stimulus, and the free yielding to responses evoked by this stimulus, constitute a basic emotional submission response.

Where may we find instances of pure emotional submission response in adult human behavior? This is some-

what difficult, as I shall try to show in a subsequent paper, since submission and inducement responses become combined in a definitely intermingled love pattern in adult behavior. The reaction of a normal woman to a 'cute' baby, however, seems to show a pretty clear submission response picture. The baby's weakness, its helplessness, and the similarity of all its reactions, on a miniature scale, to those of the woman herself, constitute a remarkably cogent submissive stimulus. At once her emotional submission response appears in the 'softening' (relaxation) of facial expression, bending over or kneeling beside the infant, and reduction of adult speech energy to produce sounds known as 'baby talk,' as closely resembling the baby's own coos and gurgles as possible. The baby, in this case, is so much weaker than the subject that he cannot possibly resist her, and all his responses, together with corresponding reactions evoked from the woman, are closely in harmony with her own natural resting reflex equilibrium. The woman responds to this submissive stimulus by easily observable reduction of tonic energization all over her body, and by freely yielding to reactions tending to make her own behavior as much like that of the baby as possible. Thus even in adult behavior, when the subject possesses sufficiently developed submission emotion, *a simple submissive stimulus evokes a basic emotional submission response.*

4. *Inducement*

Besides simple submission to the stimulation imposed, there seems to be inherent in the infant's 'love' behavior, as described by Watson, a more active response element. This may consist, at an early age, of spontaneous holding out of hands or feet to be tickled. At a somewhat later period, apparently, the baby may embrace mother or nurse. Holding out of the arms toward the person to whom the child has been submitting, and certain infant vocal sounds which might be interpreted as invitations to continue previous petting may also be listed in this more active category of love behavior.

If our preceding analysis of submission mechanism is correct, then we find that the petting or submissive stimulus (withdrawn prior to the behavior now under analysis) had been evoking certain gentle, mild reflexes which succeeded in inhibiting motor impulse opponents of the natural tonic equilibrium, and allying themselves very closely with this natural constant motor discharge. In proportion as these allies eased muscle tension, the tonic reënforcements were seen to be lessened, giving the true submission response. Now, however, with a withdrawal, or cessation of such allied impulses, we must suppose that the ordinary opponents of the body muscles (gravity, atmospheric pressure, etc.) reassert their influence, as opponents of the weakening submission reactions which had originally overcome them, and thus increase, *pari passu*, the tension of the muscles once more. This must again bring forth tonic reserve energy, this time to counteract and combat the renewed resistance to the submissive reactions. That is, the tonic reënforcements called forth by diminishing of submission reactions will be used to reënforce these very reactions.

The nature of the responses thus reënforced we have already seen; that is, gentle placing of the self at the disposal of the submissive stimulus. So long as this yielding up of the self was of a passive nature, consisting only of allowing free play to gentle reflexes evoked by the stimulator, without tonic reënforcement, we termed this response submission. Now that these responses are backed by newly released central energy, a wholly new type of integrative phenomenon appears. This new type of central energy release may be termed *inducement response*, since it is calculated to reënforce submission reactions in order to *induce further submission from another individual.* In the case of the infant, as we have seen, we find a more and more vigorous petitioning for the mother's attention. These inducement reactions become more and more emphatic and pronounced in direct proportion as the mother withdraws or decreases her submissive stimulation of the child.

Such an account of the psychoneural mechanism of a

basic, emotional inducement response is difficult to hold in mind only because males are not accustomed to think of submission to another person as being a powerful weapon in the native human behavior equipment. Unless a response can forcefully compel desired action from enemy or lover, by undiluted domination of that individual, it is thought of by males as weak. Women, however, seem always to have realized the power of submission, quickened by central energy reserves into active inducement response, as a means of influencing the behavior of others.

Inducement response seems often to appear as an unlearned type of reaction in the flirting behavior of little girls three to five years old. In boys of similar age, the reaction may also appear spontaneously, according to my own observation, in the form of aggressive teasing of attractive little girls, or smaller and younger boys. In the case of male children, however, initial submission to the submissive stimulus of a weaker child is minimized, the active soliciting of the other's submission is far more pronounced, and the whole response is apt to become mixed with and controlled by dominance very soon after its initiation, often taking the form of torturing weaker children and animals. Males, even at a tender age, appear to place little confidence in the efficacy of *inducement*, lapsing readily into attempted *compulsion* or *domination* of other humans, evidently failing to distinguish them, to any great extent, from inanimate objects.

The inducement response, moreover, seems to furnish us with the underlying mechanism of the familiar adult sex picture wherein the attracting female first arouses submission in the male by partially submitting to him; then withholds or withdraws her own submission, arousing violent efforts on the man's part to induce her to submit to him once more. This appears to constitute definite inducement behavior on both sides. The female, through submission, starts motor energy flowing through certain definite paths in the male's organism. Then by withdrawal, she furnishes resistance which *induces* energy reënforcements in these same paths.

The man, activated by these motor reënforcements, offers himself to her with all the vigor at his command. If he is successful, then he, in turn, *induces* reënforcement of the woman's original submission responses to himself. The experience is a well-known one to all of us; it is the active effort, or set to please a loved or admired person, and win her (or his) favor. We cannot compel the desired approval, we can only *induce* it by very active stimulation of the admired one with various representations of our own virtues placed at her disposal.

The word *inducement*, as a term descriptive of this seemingly basic emotional response, gains still further emphasis from comparison of its mechanism with that of the induction coil, wherein a secondary current of electricity is *induced* by a primary current, so that it appears spontaneously, as it were, without the directly conjunctive series of cause and effect sequences involved in ordinary current transmissions. As a suggested primary emotional response type, *inducement may be defined as central release of additional motor energy directed toward drawing forth, or inducing submission responses from another individual. Adequate stimulus to inducement response may be defined as inducement behavior of an individual to whom the subject is already in submission, consisting of that individual's resistance to the subject's submission responses.*

IV. Summary

A technique is proposed for psycho-physiological analysis of basic emotional response types in adults. This technique necessitates postulation of tentative primary emotional responses with definable psychoneural characteristics. Four such are suggested as a result of preliminary research.

1. *Dominance*
   - Stimulus: Domination of reaction by stronger antagonist.
   - Response: Strengthening of reaction to dominate antagonist. (Dominant response partially blocked by its own over-intensity is *rage*.)

2. *Compliance*
   Stimulus: Compliance of object with organism by adequately stimulating its existing reflexes.
   Response: Strengthening reactions elicited by object to comply with object by reacting to it without seeking to change its nature. (Compliant response partially defeated by over-intensity of compliant stimulus is *fear*.)
3. *Submission*
   Stimulus: Submission by weaker individual to control of responses by subject.
   Response: Weakening of reactions to permit their control by another individual.
4. *Inducement*
   Stimulus: Inducement of subject to yield control of his reactions to another individual by that person's giving up control of subject's responses.
   Response: Strengthening other individual's control of subject to induce other to yield control of his reactions to subject. (When inducement is used to make possible submission, *love* results.)

It may be noted that such description of basic emotional response mechanisms as above suggested does not in the least depend upon identity of resulting bodily changes, much less upon identity of sensory perceptions of such changes. Nor do the primaries herein suggested depend upon the existence of undemonstrable brain 'patterns,' or 'instincts' which must be supposed to come ready made from out the parental germ plasm. Their invariability must depend solely upon the identity of the basic laws of integrative action of the central nervous system, in various individuals and species; and upon a similarity of the natural reflex equilibrium, and its mechanisms of reënforcement and diminution.

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DRIVE: A NEGLECTED TRAIT IN THE STUDY OF THE GIFTED

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1. *Some Opinions in Reference to Gifted Children.*—There has been in recent years considerable careful study of mentally superior children. The term 'genius' has been applied by Terman and his co-workers to children possessing I.Q's. of 140 or above (23), (30). Genetic studies of such children have been made by Terman, Hollingworth, and others during the last decade. Hollingworth states that it seems best not to describe children of high I.Q. as geniuses but to substitute 'gifted,' the term previously suggested by Whipple. "Gifted children are those identified by mental tests as very superior to the average" (1). The assumption is set forth clearly by Hollingworth that gifted children actually can be selected by means of mental tests. The validity of this assumption rests upon decision as to what mental tests really measure and upon decision as to what general intelligence actually is.

The existing confusion in both regards is ably demonstrated by Freeman in his recent book (2). However many workers agree that gifted children can be identified with considerable precision and accuracy through results of mental tests. Hollingworth reveals a rather extraordinary optimism in her summary of the methods employed in selecting gifted children:

> The new knowledge thus accumulated, and the new tests invented in the twentieth century, afford us the modern approach to the study of the gifted. We can now study gifted children and *learn eventually all we care to know about extraordinarily able persons, their education, and their place in civilization* (3). (Italics ours.)

Hollingworth maintains that the children now accelerated in mental development will manifest accelerated mental de-

velopment in future years; that fairly sound prognoses of future mental development of children can be made upon the basis of their present status. Hollingworth states:

> From data so far collected, we can only say that the I.Q. is certainly constant within narrow margins . . . (4).
>
> Psychologists no longer doubt that it is now possible to predict when a child is six years old, what his relative position will be in the total range of intellects when he is sixteen (5).

Hollingworth assumes that gifted children become gifted adults and further that the world's goods are enhanced greatly by the contributions from the minority possessing great innate mentality. "Individuals of surpassing intelligence create national wealth, determine the state of industry, advance science, and make general culture possible" (6). Identification of and subsequent special educational provision for gifted children therefore are social obligations.

Hollingworth is not alone in urging early identification of gifted children with subsequent special educational provision adapted to their special needs and abilities. Rugg states that American schools are now responsible for the training of approximately one million children of distinctive ability.

> Altogether American public schools are now responsible for the training of upwards of one million children of distinctive mental ability. . . . Such children . . . have come to be called 'gifted.' They have such 'abstract intelligence'—that is, such ability to manipulate ideas expressed in words—that they graduate from the high school at the age of 15 or 16. . . . With their unusual verbal mental ability they take school and college honors. They tend toward academic occupations, at least towards occupations demanding analytical ability. . . .
>
> Some of these verbally intelligent thousands in our schools—perhaps between a third and a half (I personally subscribe to the smaller proportion)—are not only 'gifted' in verbal intelligence; they possess distinctive social qualities also. These belong to another army of 'gifted' persons for whom the school is responsible, our *socially* talented. The *socially* gifted pupils are now the 'leading citizens' of a school or college; it is probable that later in life they will also be the 'leading citizens' of some community. . . . As adults later some of them will become the mayors of towns, the

governors of states, executives in business, presidents of chambers of commerce, organizers of Rotary and Kiwanis clubs. . . . We must not forget the mechanically gifted or the esthetically gifted. . . . An interest in mechanical manipulation coupled to mathematical and scientific ability will make out of some of the 'mechanically gifted' the Steinmetzes, Marconis, Langleys, Howes, Goodyears, Bells, Morses, Edisons of the next generation. . . . (The) esthetically 'gifted' young people in our schools . . . *will be* the masters of the American culture of the next generation (7). (Last two italics ours.)

In the above quotation Rugg states that gifted children *will* become the future leaders. Hollingworth writes with greater caution, "We cannot know with certainty however until a lifetime has elapsed whether they (gifted children) will really become the eminent of their generation" (8). However, biographic reference to Benjamin Franklin, Marie Curie, André Ampere, Gauss, and others are adduced by Hollingworth to demonstrate that precocity in mental ability was manifested in early years by certain individuals who later achieved greatness. The parallelism is drawn between the early years of these eminent individuals and the early precocity of gifted children.

2. *Capacity versus 'Drive.'*—The optimism with which Rugg, Terman, and Hollingworth look upon the future of gifted children is frankly not shared by the present writers. They feel that the importance of capacity is over-emphasized and the significance of drive neglected or under-estimated. The fundamental questions involved are: (1) Is drive to activity dependent largely upon inherited ability or is it a product largely of certain attitudes effected by one's varied responses to his environment? (2) In a given individual or group is drive proportionate to capacity as indicated by the Binet tests? In other words, is a test of intelligence also a measure of an individual's drive?

In the opinion of the present writers drive is a product of numerous variables; it is largely the result of conditioning. If this hypothesis be correct, it follows that ability in itself does not insure intense effort or drive. The preceding

hypothesis is substantiated by certain known facts. For example, there is frequently a discrepancy between E.A. (educational age) and M.A. (mental age) for children of high I.Q. The writers have observed an individual of I.Q. well above 140 who exhibited only the average educational attainment of children of his C.A. In this instance, the ability seems to be present, the drive lacking.

3. *Factors which Influence Drive.*—Among the many elements which effect 'will to do' is the need for compensatory behavior on the part of the organism unable to obtain gratification in various other lines of endeavor. The principle of compensatory activity is a familiar one to the student of psychology. In a recent article entitled 'The Psychology of Compensation,' Vaughan makes the following statements:

> An individual who realizes that he is deficient in one field sets out to conceal that incapacity by training his ability in another direction. The weakling becomes a scholar, the plain woman develops a pleasing disposition, the bore practices thoughtfulness of others. Socrates made up for his homeliness and domestic indolence by cultivating a beautiful soul. When a man fails to succeed along general lines, he turns to the training of his special abilities as a refuge (9).

Adler too has commented upon the force of inferiority in actuating various forms of compensatory behavior. He states:

> Somatic inferiority determines through the accompanying psychic phenomena of repugnance the direction of the ideas of pleasure and thus conducts the compensatory processes into the psychic regions. Here, too, we behold the craving for security at work and usually in such a purposeful manner that it works coefficients which offer security and thus gives rise to over-compensation. In the development, for instance, of the stuttering Demosthenes to the greatest orator of Greece; of Clara Schumann, who was deaf, to an accomplished musician; of the nearsighted G. Freitag, of many poets and painters with anomalies of the eye to visually talented persons; and of the numerous physicians with anomalies of hearing, we perceive the result of the craving for security (10).

Adler is not alone in attributing great driving force to somatic inferiority. In a recent biography of William Hohenzollern, Emil Ludwig asserts that William II.'s fondness for uniforms, parades, sabre-rattling, and aggressive display of authority were actuated by a desire to compensate for a congenital inferiority. From birth the emperor's left arm was paralyzed.

"An incurable disability in the left arm," writes his tutor, Hinzpeter, "was a very particular hindrance to his physical and psychical development. . . . He was confronted with the task of overcoming a natural sense of bodily helplessness and the timidity inseparably connected with it" (11).

William was driven to an ostentatious display of his authority by the wish, perpetual still, even though perhaps unconscious, to betray no sign of physical weakness (12).

The perpetual struggle with a defect which every newcomer must instantly perceive and he, for that very reason, the more ostentatiously ignore, this hourly, life-long, effort to conceal a congenital, in no sense repulsive stigma of nature, was the decisive factor in the development of his character. The weakling sought to emphasize his strength. . . . Tradition and vain glory urged him to the exhibition of an heroic, that is to say, a soldierly personality (13).

The case of Steinmetz is pertinent in this regard. It is a well-known fact that Steinmetz was handicapped physically by a crippled body. The extent to which this condition served as an incentive to Steinmetz's later achievement is of course unknown. It seems probable however that this physical condition may have been a potent factor in *determining the direction of his energy and the nature of his achievement.* It is important to recognize that a physical anomaly often leads to redirection of energy, intensity of effort, and consequently to outstanding attainment. The causes for compensatory behavior when physical anomalies are present are sometimes easily discernible. But the multitudinous and diverse forms of substitute behavior occasioned by mental anomalies or maladjustment are less easily identified and consequently often elude analysis.

Of conspicuous importance is substitute activity, compen-

satory in nature, in conditioning achievement in imaginative fields. A recent American biography of Edgar Allen Poe points out that Poe was driven to seek a position of literary eminence by a desire to compensate for a precluded social position, denied sexual craving, and a knowledge of mental instability.

> Stung by the humiliation born of some sense of inferiority connected with the inhibition of his passions, and enormously irritated with the sufferings which he had undergone as a ward of Mr. Allan, Poe sought in gratified pride a balm for his feelings; but this balm was most soothing when recognition was not begged but demanded (14).
>
> The compensation for his wounded sense of pride, sprung from the humiliations of his childhood and aggravated by his psychic impotence, appeared first as only an exaggerated ambition (15).
>
> Just as his whole life was a struggle, conducted with all the cunning of the unconscious, against a realization of the psychic impotence of his sexual nature, so was it a struggle also against a realization of the mental instability to which the first gave rise (16).

Mr. Krutch asserts that if what he says of Poe is true, it is plausible to assume that *all* imaginative work is the result of human maladjustments to life. This hypothesis set forth by Krutch in reference to the actuation of works of genius in imaginative fields of course needs validation. That the desire for compensatory satisfaction is the *only* factor or the *most important single factor* in determining genius in imaginative work is yet to be demonstrated. However, the assumption seems warranted that this desire is *one* factor of considerable importance.

Of interest in this connection is Schopenhauer's quotation from Aristotle, "Men distinguished in philosophy, politics, poetry or art appear to be all of melancholy temperament" (17). Schopenhauer's own opinion in reference to this matter is set forth in the following: "The direct connection of madness and genius 'is established by the biographies of great men, such as Rousseau, Byron, Alfieri, etc.'" (18).

Will Durant implies that mental instability or madness is connected with the romantic type of genius though not necessarily with genius in general.

Is madness connected with genius in general, or rather with only the 'romantic' type of genius (Byron, Shelley, Poe, Heine, Swinburne, Strindberg, Dostoievski, etc.); and is not the 'classic' and profounder type of genius exceptionally sound (Socrates, Plato, Spinoza, Bacon, Newton, Voltaire, Goethe, Darwin, Whitman, etc.)? (19).

However, there are instances in which madness has been connected with other than the romantic type of genius. Nietzsche, the philosopher, ultimately became insane and certain psychoanalysts insist that Nietzsche's genius was in reality a struggle against realization of his own mental instability. Similarly, Castlereagh, dominant figure at the Congress of Vienna, ultimately went mad. The outstanding endeavor of these men may be traced at least theoretically to impelling motives to escape certain unpleasant realizations. The nervous instability itself provides sometimes an active agent for unusual performance.

MacLaurin has taken a position similar to Vaughan, Adler, Krutch, and Ludwig. MacLaurin has made an attempt recently to explain the actions of certain remarkable historical characters, not on the basis of high I.Q.'s, but on the basis of abnormal mental and physical conditions effecting the deviation in performance. In introducing his work the author states:

> There are many remarkable people in history about whom we know too little to dogmatize, though we may strongly suspect that their mental and physical conditions were abnormal when they were driven to take actions which have passed into history, for instance, Mahomet and St. Paul. Such I have purposely omitted. But there were far more whose actions were clearly the result of their state of health, and some of these who happen to have been leaders at critical epochs I have ventured to study from the point of view of a doctor. This point of view appears to have been strangely neglected by historians and others (20).

A single example is sufficient to illustrate the type of explanation advanced by MacLaurin. He attributes the leadership of Jeanne D'Arc to her abnormal bodily condition.

> But Jeanne (D'Arc) had a body, which was subject to the same

peculiarities and abnormalities as the bodies of other people; and upon the peculiarities of her physiology depended the peculiarities of her mind. . . . Jeanne never menstruated. . . . It is especially significant that she first heard Voices when she was about thirteen years of age, at the very time that she should have begun to menstruate; and that at first they did not come regularly, but came at intervals, just as menstruation itself often begins. . . . Probably the delusion as to the Voices first began as a sort of vicarious menstruation; probably it recurred when menstruation should have reappeared; we can put the idea of virginity into the jargon of psychoanalysis by saying that Jeanne had a well-marked 'repression of the sex-complex' (21).

The above description of Jeanne D'Arc's experiences strongly reminds one of St. Paul's testimony to the effect that he possessed bodily infirmities, that he maintained continency, and that he had seen visions (22).

One could easily add other examples of nervous instability and conspicuous attainment being associated. Certainly it seems that the hypothesis set forth by certain workers that children of high I.Q. are well-adjusted children and that they are at the same time our future geniuses needs validation.

4. *Post Mortem I.Q.'s.*—Recently, there has been reported a notable attempt to assign I.Q.'s to men of genius long since deceased. Historical records showing childhood traits were utilized as a basis for assigning intelligence ratings (23). Thus, Napoleon and Beethoven received ratings of 135, Byron 150, Michaelangelo 145, etc. Cox in this study speculates in reference to *innate ability* and assumes relative constancy of the I.Q. It would be equally valid, n the writers' opinion, to speculate upon the *drives* to activity which conditioned the outstanding performance of various historical characters. Indeed MacLaurin has done this very thing. He has assembled an imposing list of abnormal and physical conditions which he claims to have been potent forces in effecting the behavior of historical characters. Who shall say that the Cox speculations in reference to the innate abilities of the historical characters are any more valid than a parallel list of speculations in reference to the

drives that influenced these individuals to intense effort and consequent achievement? It is obvious that the Cox *post mortem* I.Q. assignments must rest upon the assumption that a high I.Q. is essential to conspicuous attainment and the correlative assumption that conspicuous attainment posits a high I.Q. (23). Needless to say, the first of these assumptions has never been demonstrated, and until it has been validated by unmistakable evidence both it and its corollary remain pure speculations.

5. *A High I.Q. Does Not Invariably Posit Drive.*—In view of the conflicting opinions of Cox and MacLaurin in reference to the stimulation of outstanding achievement among historical characters, it is interesting to examine the results of the studies of gifted children with the importance of drive clearly in mind. These studies have demonstrated that the child of mental precocity usually has no somatic defects for which he must compensate. He is not typically of lop-sided development. Indeed, Davis states in reference to the gifted child:

> Contrary to the popular belief current some ten years ago, all these studies indicate that as a rule the unusually capable child, instead of being a one-sided product, overdeveloped in one or two respects at the expense of all others, is really an all-round individual, not only highly endowed intellectually, but superior in moral, social, and personality traits as well. Cases in which these conclusions are not borne out are conspicuously few (24).

Terman, too, emphasizes the general superiority of gifted children:

> The data show that the physical and mental traits which characterize typical gifted children appear to warrant the belief that more intense educational effort in their behalf on the part of the school would not involve the risks which various writers have feared. *The 'eccentricity of genius' is largely a myth* (25). (Italics ours.)

The general adequacy of adjustment of the gifted child is illustrated by the following:

> . . . Gifted children usually conform more readily (than the

average run of children) to the moral and ethical standards of society . . . (26).

There is at hand sufficient experimental evidence to show that superior children mingle easily with other children, and as a rule show little difficulty in adjusting themselves either to an older or to a younger group (27).

The point here insisted upon is that the gifted child (I.Q. 140 or above) is typically well-adjusted and is therefore unlikely to be driven frequently and tenaciously to seek out satisfying activities. The available data disclose little need on the part of the gifted child for compensatory behavior of the kinds emphasized by Adler, Krutch, MacLaurin, and others, since in most instances he is superior physically, socially, morally, and economically, to the average run of children. The question therefore arises: Will present-day gifted children ever be actuated sufficiently to accomplish outstanding work? Certainly, the presence of a high I.Q. is not a guarantee of the presence of drive.

Swift has listed a large number of historical characters who at first gave little promise but who subsequently became eminently successful in various lines of endeavor. Two examples will suffice to present the situation.

Henry Ward Beecher at ten years of age, according to his sister, Mrs. Stowe, "was a poor writer, a miserable speller, with a thick utterance, and a bashful reticence which seemed like stolid stupidity." . . . He was not marked by the prophecies even of partial friends for any brilliant future. He had precisely the organization which often passes for dullness in early boyhood. He had great deficiency in verbal memory, a deficiency marked in him through life. . . . In forecasting his horoscope, had any one taken the trouble then to do it, the last success that ever would have been predicted for him would have been that of an orator (28).

Two small boys were rivals for the last seat on the last bench in their school room. It was difficult to tell which was the poorer scholar, because they both stood so low and were running, most of the time, neck and neck. When in after years, these two dunces met, one of them Wilhelm Reuling, had become leader of the Imperial Opera in Vienna, and the other, Justus von Liebig, a chemist of world-wide fame. Liebig himself has told how the

director of the gymnasium that he attended once visited his class, and, after observing his wretched work, told him that he was the plague of his teacher and the sorrow of his parents, and asked him what he thought would become of one so inattentive and indolent. When young Liebig replied that he would become a chemist, the director and school burst into an uncontrollable fit of laughter. Liebig's classwork continued so unsatisfactorily that his father was obliged to remove him, and in despair, apprenticed him to an apothecary, but after ten months, as Liebig says, the pharmacist grew so tired of him that he sent him home. Liebig has suggested one reason for his poor success in school. He had scarcely any auditory memory and could retain little or nothing of what he heard (29).

It is of course mere speculation to assume that the genius of the above individuals could have been foretold by means of mental tests. In view of the fact that Swift is able to cite many other historical characters who at first gave little promise but who were subsequently able to become eminently successful in various lines of endeavor it seems probable that certain of our more recent writers have been premature in their assertions that gifted children *will achieve* (7), and that "the extraordinary genius who achieves the highest eminence is also the gifted individual whom intelligence tests may discover in childhood" (23).

In this connection it is interesting to observe that Terman has recently isolated 643 children whom he designates 'geniuses' (30). It is possible that Terman has succeeded in identifying children who have both the capacity and the will to achieve; children who will ultimately be acclaimed geniuses. It is possible, on the other hand, that the wish has been father to the thought and that Terman has been premature in proclaiming to an expectant world that the children whom he has been studying are geniuses. The validity of the tests as a means of predicting genius must be ascertained *empirically* if it is to be ascertained at all. It is incredible that the writers quoted should have attempted to establish the validity of the tests *a priori*. Follow-up studies are eminently worth while but time alone will enable us to know what the future development of the gifted will be.

It seems to the present writers that the only rational position to take now in reference to the future of 'gifted' children is that of suspended judgment.

6. *Drive Only One of Many Elements Conditioning Outstanding Achievement.*—The reader must not assume from the above discussion that the present writers recognize in the desire for compensatory gratification the *only* drive toward outstanding endeavor. Drive is conditioned undoubtedly by multitudinous factors; the desire for compensatory satisfaction being only one element of considerable importance among many varied forces which condition the will to accomplish. In view of the present-day optimism in reference to the gifted child it seems necessary to the writers to recognize the importance of drive in determining genius in *actual functioning* and to emphasize the possibility of works of genius emanating from individuals of moderate capacity, who possess however impelling drives to accomplishment. Certainly, capacity is a requisite to accomplishment but 'will to do' or 'desire to accomplish' likewise must not be overlooked. It is obvious too that intelligence of a high order may or may not be accompanied by such desire to accomplish. It has already been shown that the literature regarding gifted children reveals little need on the part of these children for compensatory behavior and no evidence has as yet been advanced to support the assumption that the intelligence test is also a test of drive, *i.e.*, that great driving power accompanies high intelligence always or even generally.

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GENIUS AND THE I. Q.

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To the individual speculating on aspects of mental behavior in humans there are probably few phases that are more intriguing and baffling than that which befogs the issue of *what is creative thought.* Terman intimates to us in his earlier work on tests that it is essentially I.Q. In the recent studies on genius the Stanford group seems more willing to admit that there is something more than I.Q. It is I.Q. plus. Plus what? In this they make us very little wiser. It is unfortunate that this serious and important group of workers in the field of intelligence testing should have found themselves so restricted in methods of attack. This statement is intended in no way as a destructive criticism, but is made merely that points, later to be introduced, may be more adequately developed. Essentially, they are employing two methods, with modifications of course, for the collection of data in their investigations of genius. These are the method of Intelligence Tests and the method of Ratings. Let us examine them briefly.

Intelligence Tests need little discussion. They do give quantitative expressions of differences in persons. They are diagnostic, within limits, in at least some phases of human endeavor, *i.e.*, school performance. These quantitative expressions in terms of I.Q. are relatively constant. It could be argued that the I.Q. is an artifact, some fault of the scale or what not, but there would still remain the fact that, except in isolated cases where improvement is due to the correction of gross physiological or emotional disturbances, or where deterioration sets in again as a result of some general physiological disturbance, such as myxœdema, the I. Q. shifts very little. As Thorndike has put it, the performance fluctuates around a certain mean. Whether the I.Q. is

constant or not—Kuhlmann suspects it is not—there is brought out through intelligence tests the fact that there is some sort of a constant. The individual occupying a certain rank on the basis of his performance within a given age group will occupy, relatively speaking, approximately the same ranking in an age group two or three years hence. From this it is argued that since the I.Q. does remain constant; since, empirically, it has been shown sufficiently often to have a fairly close relationship to school performance; since, from time immemorial, academic or learning performance has been taken as a fairly exact measure of mental ability; and since this empirical procedure has been justified by careful studies between school ability and later success in life—Galton and others—we may safely assume that whatever the tests test they are fairly close to the problem of intellectual ability. In other words, measured against the most adequate manifestation of intelligence that we have in a child, school performance, they stand up.

But is this entirely true? It is our hope that we may be able in this paper to point toward some possible means of studying the problem that might enable us to bridge somewhat the gap that exists between the child's present performance and his performance as it *may* be twenty or thirty years hence. The Stanford people are quite correct in pushing their generalizations as far as possible. It is legitimate and desirable from a methodological standpoint that the generalizer project his generalizations as extensively as he can. This does not mean, of course, that they will stand the empirical test of time. It merely means that such a procedure may be effective in speedily precipitating the main issues, and through this elimination clear the deck for the attack from the strategic quarters. We should, however, be cautious about thinking that ultimate fact has been stated simply because the method by which the data were collected has been sustained by another method, particularly if the latter be less precise than the former. This, we think, in justice, can be said of the rating method. Essentially, the rating method is little more than a modified questionnaire

method. We do not know how much emotional bias, determined by our social habits of thinking, enters in to color most ratings. We do know, and have known for a long time, that if the rating method is used at all, it must be done with considerable wariness. Yet, on the other hand, ratings have this distinct advantage that, taken as a whole, they tend to leave the particular traits under investigation in their complex state and they do not become, when experimentally scrutinized, the *meaningless dry rot material* of so much of the experimental content of psychology. Separate a trait from the total situation of the normally reacting organism, peel off its variables one by one, and there remains little but a *something* that has little likeness to its original nature and cannot be synthesized. In so far as methods tend to treat a function in its complex and natural state, I believe the result may be salutory, not because of contributions in the way of basic facts, but because such methods may get us closer to the position where we may understand *why* and, perhaps, even *how* human beings behave in their complex activities. The rating method may be productive in this direction.

This is precisely where the Stanford group seems to have resorted to questionable procedure. Having arrived at a quantitative expression, the I.Q., which remains relatively constant for the individual, an expression that has clearly limited the word *genius* and given it a meaning that, expressed in terms of test performance, means the same to all working in the field of psychology and a great share of the general public at large, these people then try to sustain this concept, with its short range of latitude, through the study of biography. This is not a review of the second volume of the *Genetic Studies of Genius;* neither the space nor the inclination permits. It is merely an attempt to point out what may be, I do not say are, some of the restrictions of the previously stated concepts and to try to scrutinize the problem from two other angles, the adult intellectual performance angle and the naïve performance angle of the young child.

Let us take as an illustration of the first a graduate student

who is mustering originality to the attack of his problem. Speaking again empirically, usually a dangerous procedure, are we always confronted with the spectacle of the *test breaker* doing the most promising work? Any director of graduate students has had the experience time after time of being invited by a high test order student to hand him a thesis on a salver. The student very considerately reserves the clerical privileges for himself, and yet, such students, once they are started, often *grow on the job.* They do exceed expectations and show some originality; they do see the total aspects of their problems and cover them adequately; they devise methods; they discover new problems to be attacked later; and a few grow, or their latent talents simply emerge into that rather rare and happy state, *creative research ability.* But for one that comes through at least five are still-born soon after they pass their finals. If we could say why, we would probably know far more about genius than the I.Q. or the studies in biography will ever tell us. We languidly rest on our oars by saying that, although the five have the intelligence, they are lacking the proper personality traits. Perfectly true, perfectly general, and perfectly meaningless!

It is then perfectly clear that whatever genius in research directions is, it is something more than I.Q. A high I.Q. may be one of the necessary attributes, but it is only one. There must be in addition that divine discontent—perhaps stupid discontent—, that somewhat naïve faith in the efficacy of human effort over and beyond the demands of one's job, that ability to take directions and maintain them enthusiastically, and many other things. But of these it often seems in the effectively productive person, gauged by quality as well as quantity, that a general harmony, a set if you please, is the outstanding characteristic, and this, to a considerable extent, smacks of an emotional orientation that is so harmonized with the rationalizing capacities that it brings them into their most effective operation. Indeed, it even seems that the highest productive activity is made possible only through this emotional control. The individual who sits calmly in the sun and *thinks* probably very often ceases even to think, at least on a plane of high order.

And yet, tacitly, in the concept of the I.Q. and the theory of intelligence tests in general, this emotional dominance is ruled out. Were it not, it is hardly conceivable that the I.Q. could remain as constant as it does. If emotional organization is not synonymous with reasoning or an integral part of it in the same proportions, it would seem that it would have to manifest its presence by disturbing the I.Q. constancy. The only other alternative, if the above thesis is correct, is that the I.Q. be dependent on conditions in which the emotional tone has been excluded. This, however, forces us into a dilemma. In, perhaps, the highest type of intellectual activity which we can observe in the academic environment, creative work, research, and other forms of production, this emotional tone is present, outstandingly so.

When we want a piece of work evaluated, we frequently say that we want *unbiased opinion.* We want nothing of the sort! We want, instead, distinctly biased opinion, at least to the extent that the people evaluating will have had sufficient interest to have informed themselves well in the field with which the research purports to deal. For that reason the behaviorist, at least in the technical aspects of his problem, takes more seriously the criticisms of his fellow behaviorists than he does, say, those of a structuralist. Facts are cold, uninteresting stuff except in the light of some hypothesis. It is the hypothesis that puts warmth in the data—too much at times—and the hypothesis is essentially a very personal thing. It is to the hypothesis that the individual musters his maximum capacity in creativeness. It is his creativeness! Further, it is distinctly colored by the feeling tone. A research is nothing more than a series of hypotheses. Every technical difficulty overcome is merely a hypothesis in relation to the original and dominant problem, and that investigator is great who is able best to be fluid and flexible in the formation of antagonistic concepts with which to test the dominant hypothesis. This is all science ever demands of its devotees. Why then, in the presence of living, dynamic, creative workers may we not get at the problem of genius as effectively, if not more so, as by the

study of biased, untruthful, and incomplete biography? It could be done just as easily, just as effectively as by the I.Q. biography method, provided we are willing to scrutinize our gods for a while. This does not mean we have to abandon mental tests and I.Q.'s. It merely means that we must scrutinize the I.Q., not from the knee position of a god to be venerated, but from the assumption that it is a peculiar imp with many traits to be investigated. It manifests a sort of magician's skill in concealing its tricks.

How much of memory, attention, perception, go into intelligence tests? How much of this *personality drive* is the result of inheritance or of physiological maladjustment? We have clinical cases in support of the latter, for example myxœdema. How much can thinking be divorced from feeling, and if it is, is the result the highest type of creative product? Adult creativeness militates against this hypothesis. But it will likely be said that *that is precisely what psychology was trying to do before Binet. It never succeeded.* True! But could it not be said that it might have succeeded if Binet had been born a hundred years later? No one will attempt to deny the contribution of Binet, a contribution which is one of the most outstanding that psychology can offer; but, on the other hand, need damnation center from so many quarters merely because one voices his discontent with the fact that, since the tests have so stubbornly resisted analysis, we have stopped trying to *understand* them and merely *use* them.

In the test data that have come from pre-school children it seems at times that the advocates of tests have failed to take into account the total implications of their results, some of which seem to distinctly support their view. An I.Q. established on a pre-school child is predictive within limits of what that individual's I.Q. will be at ten. Yet the contents of the test material at the different ages are quite different. In the levels for the pre-school child they are little more than a jumbled mass of chronological age norms. Why should the appearance of certain reflexes at certain times be predictive of mental potentiality? This has never been adequately answered. The *mental growth curve* with its implied

companion, *static neurological structuralism*, does not even satisfy the neurological facts. Comparative Anatomy emphatically teaches us that that part of the nervous system concerned with the higher mental operations is phylogenetically young and comparatively flexible. On the other hand, here are activities in the very young child, controlled by tissue phylogenetically very old, which are predictive of high mental operations eight or ten years hence. This fact should at least be mentioned in the theory of tests as it argues for their validity.

If the I.Q. is the *valid index* of mental ability, and if that mental ability is an expression of nervous functioning, then it would seem that this constancy is expressive of a general potentiality in functioning. This can not easily be explained except in terms of an hereditary determinism, an explanation which is rendered all the more easy by data coming from other sources. It does put the I.Q. in a position of defense. Either the neurological generalizations, which have come in the main from comparative anatomy, must be qualified or the I.Q. is less significant than we think. This is the position which I previously indicated might be able to be attacked from the standpoint of the performance of the young child. We must cast his performances against what we now know of nervous functioning, pitifully small as it is, and either they must be harmonized with these neurological concepts or one or the other will have to go. In other words neurology teaches us that *old* and *new brain* behave differently, yet the child's performances dependent on these tissues indicate this is not true.

To conclude, limit the I.Q. concept as the Stanford group has done in the studies of gifted children and creative men of history, and what have we? An I.Q. of 150 and we have *genius*. Genius has a high I.Q. True, but a high I.Q. does not necessarily mean high creative productivity. It aborts itself in a most depressing way in every graduate school in the country, and to the watching individual, there is always present in the lucky few who possess this *plus quality* something that seems to be absent from the intelligence tests,

which should be there if they measure what they purport to measure. There can be little doubt that there is a distinct gap between the empirical results which have come from tests and the equally real empirical observations, which tend to suggest that adult creativeness is the result of an intellectual mobilization, which, if not emotionally driven, is at least facilitated by feeling.

THE MEASUREMENT AND PREDICTION OF MENTAL GROWTH

BY ARNOLD GESELL

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Embryology is the cardinal science of development. Broadly conceived, embryology embraces not only anatomy, but all knowledge which bears directly upon the phenomena of growth.[1] In a schematic sense, even developmental psychology falls within its scope.

Wilhelm His, one of the founders of this science, held that "The ultimate aim of embryology is the mathematical derivation of the adult from the distribution of growth in the germ."

How is that derivation to be achieved? Can it be achieved at all in the psychological field, in the embryology of the mind? One cannot take too much for granted. Is it possible that development, in the mental sphere, is not in itself a valid scientific problem? It may be a philosophical conception which has its place in the metaphysics of Heraclitus, Bergson, and Driesch, but which should be used with great scientific wariness.

Thorndike in a recent and notable volume[2] makes a hint in this direction. He says, "The great merit of the Binet test is that it is a graded scale for intellectual difficulty, and it is only weakened by being interpreted loosely as a measure of some mysterious essence called intelligence, which grows in man." Thurstone has flatly asserted "that the mental age concept is a failure." What indeed is the scientific value of the category of age in developmental psychology?

Although Thorndike discusses intellect in relation to age, he is chiefly concerned with the direct measurement of

[1] In this discussion no attempt will be made to maintain a distinction, of doubtful value, between growth as augmentation, and development as differentiation.

[2] Thorndike, E. L., 'The Measurement of Intelligence,' Teachers' College, New York, 1927, Bureau of Educational Pub'ns.

intellect as such, intellect as capacity. He proposes new methods for determining the altitude, the width and the area of intellect, and makes this significant comment: "Only one thing is needed to make such measurements submissible to the arithmetic and calculus of science in general. That is the expression of the altitude of each level (now merely a rank) as an amount of difference from the altitude of others and from some group of tasks which require intellect, but so little of it that they border on a true zero of difficulty which may be set as their lower limit. This is the fundamental problem of mental measurements." This statement expresses the positivist standpoint of absolute psychometry and must have application to many phases of quantitative technique.

The absolute measurement of abilities, however, leads to questions concerning the measurement of growth as such and invites also a consideration of the import of age in relation to mental growth. At the present stage of scientific method, it would seem that many problems of genetic psychology must still be approached in a relative manner, even in their quantitative aspects. From the standpoint of genetic psychology, and of psycho-biology, the mind as mind, capacity as capacity, behavior as behavior, cease to be of central concern and become rather the foils for the dynamic study of change in relation to duration. Duration then becomes a fundamental category. It is the growth, the alteration, the emergence, interplay and time progression of factors, which call for formulation. Mental growth makes a successful contest with zero at many different points. Or it is a process which cumulatively creates new conventional zeros and brings them into shifting relations. It is the dynamics of this process which in some way, however crude, needs delineation combined with measurement. The delineation must take fundamental account of the factor of age. The elucidation of agedness in relation to maturation likewise becomes a task for systematic research.

We therefore assume that mental growth in this dynamic sense *is* a valid scientific problem; that it may be studied in its own right, however heavily the conclusions hinge on

static and absolute data. The mental manifestations of growth must be viewed historically, in their natural sequence. It is the grouping and interaction of various manifestations at ascending stages in an age cycle which form the basis for a description of the growth complex and a derivation of growth laws.

Age and growth are inseparable concepts. All growth takes time; but age is more than duration. It is a topographical as well as a quantitative category. Any given age always represents a position in a cycle. For this reason age norms in some form are indispensable to the study of growth.

But what form shall these age norms take? How shall we calibrate age in making our serial cross-sections? Where shall we place our secular zero? Chronometry furnishes us with units ranging from a fractional thousandth of a second to a year; but it does not locate the absolute zero in the life cycle of the child, nor does it furnish any formula which will equate age-values at remote ends of the life cycle. It has long been the practice to reckon a child's age from the time of birth; but this is a most naïve convention which mocks the dignity of the true zero. Birth itself takes place at different 'times' in the biological sense. This variability of the natal zero introduces interesting and instructive complications into the study of early mental growth.

Time in the Newtonian sense flows at a constant rate. Development flows with some kind of negative acceleration. In the germinal period of the human zygote, hours count; in the period of the embryo, days count; in the period of the fœtus, weeks count; in infancy, months. Our prevailing measuring scale for the school child is notched at the years.

There is an involuted compression of development in the early stages; all strands of growth converging upon the zero point. There is an apparently uneven tapering of these strands into the zone of senectitude. As we reach maturity the age level concept becomes more difficult of application; but it does not lose its essential validity. We are still concerned with increments of growth in relation to duration,

and when the processes of development become reversed, so that increments give way to decremental decline, it is still more desirable to keep the methods of description and of measurement continuous with those applied to fœtus, infant and child.

In developmental economy the value of any given unit of duration is quite relative and is governed by the secular location of that unit in the life cycle. In early infancy the developmental significance of short intervals is comparatively great. For the infant psychologist the wrong end of the field glass is perhaps the right one, because he must view the rapid elaboration of early behavior through time lenses which will deepen perspective and restore the compressed events to their intrinsic developmental proportions.

The mechanics of growth may some day be formulated in biochemical or physico-chemical terms; but even then we shall need some quantitative logic or graphics to express the dynamic configuration and the temporal design of the total growth complex. The analysis of lines and foci of growth with reference to their interplay, their competition or autonomy, their rhythms, fluctuations, dominance, subsidence and integration, must be quantitatively or graphically portrayed in systematic relation to the factor of age. The study of the crucial problem of maturation likewise must give consistent heed to the clock and the calendar.

If our general interpretation is sound, the following propositions may be laid down. For the sake of brevity they are worded rather categorically:

1. Growth, as growth, is a genuine scientific problem, whether conceived in physical or mental terms.

2. Mental growth can be studied through systematic inventory, seriation, and mensuration of behavior phenomena at any successive points of any part of a life cycle.

3. The location and dynamic significance of the phenomena must be expressed in terms of age or its equivalent. Age is an orientational as well as quantitative category.

4. Absolute mensuration of traits and capacities is technically essential, but must be supplemented by historic and

naturalistic interpretation. Relative as well as absolute units must be used to elucidate data. Even descriptive normative formulations are valuable if they are made objective and comparative. Developmental increments may be profitably studied by the method of paired comparison of adjacent age levels.

5. Growth is a continuum-complex of variable and more or less elementary components. The constellation of components needs graphic and mathematical expression. The concatenation of components can be determined only through clinical and experimental observation.

6. Clinical and experimental observation are not mutually exclusive. Rather they are reciprocally corrective. In the complicated field of human growth they should reinforce each other. Clinical approach is peculiarly important in the study of mental growth; because growth is always a unitary end-resultant of multiple factors in one individual. Individual differences in modalities of growth must be investigated as well as more purely quantitative deviations.

7. Growth changes may be studied through the genetic seriation of increments of behavior values and a parallel seriation of decrements. A genetic order is more than a rank order and need not be reduced to absolute or uniform units to be scientifically serviceable.

Such increments may be formulated for specific functions or even for such complicated aggregations as personality. The number of stages and corresponding norms of development which may be studied approaches infinity.

8. A critical recognition of the influence of age in growth is needed to determine the stages of growth which can be most advantageously studied in the field of child development. The compression of development about the zero point places a scientific premium on the early age levels.

9. The reckoning of chronological age from the time of birth presents both theoretical and practical difficulties for genetic research. Birth marks the location of a nodal but not a true zero. From the standpoint of psycho-biology there are numerous other 'nodal' zeros, but the absolute zero for reckoning life age should be placed at conception.

10. Age norms must be multiplied as well as refined for the study of early mental growth. The rapidity of circumnatal development demands an increasing emphasis on smali units of duration at this sector of the growth cycle.

* * *

In spite of its prodigious intricacy, mental growth should yield to measurement and to prediction. Growth is without caprice. Growth is a progression which proceeds in orderly relation to age from conception to death. A scientific exploitation of the age factor will inevitably lead to some kind of exterpolation of the pattern of future growth. The refinement of this exterpolation is prediction. If prediction is the essence of science, mental growth has a better scientific status than many problems which seem more simple and more valid.

Growth is lawful. The seasons in their sequence and in their consequence are so ordered that Thoreau boasted that he could tell the day of the month within two days by the flowers that grew. In spite of its bewildering complexity, the growth of the child mind also will be found to be within the realm of law.

THE COMMUNITY OF IDEAS OF INDIANS

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As long as the American Indian remains an object of interest to the psychologist, or any one else who is of an inquiring mind and who is himself not an Indian there will always be a curiosity to know just what goes on in an Indian's mind. He looks so different and has had an heredity and tribal life so different from that of the White that one inclines naturally to think his mental make-up must be different.

Traditionally we have it that he is stoical in enduring pain and discomfort and that he is fond of color. Psychological experiments in mental fatigue alone indicate that the nomadic Indian, at least, does not drop in his efficiency as soon as the White, nor as the Negro who falls away rapidly in his efficiency, though he may not deny that he has feelings of fatigue after the experiment is finished (8). As to color, his fondness for the color red is made evident by test and the discrimination on his part of æsthetic preference for one color as compared with another is much more decided than is found to be for Negroes (9) and even for Whites who alike prefer the color blue above all the others (10).

These traits have to do with the dynamic and affective sides of life and would seem to have their roots in the springs of existence. But one wishes to know something of the Indian's cognitive states. What is his mind made up of in the way of ideas?

It is the intention of this study to look into this problem of the community of ideas of the Indian and moreover of the full-blood Indian only so as to avoid the complication of the effect of White blood on the situation.

Studies of the community of ideas were made some time ago by Cattell (1), Jastrow (2), Calkins (3), Nevers (4), Tanner (5), Manchester (6). In most of these studies it was

sought to show sex differences in frequency of various categories. Jastrow found that women contribute more words belonging to the category of 'wearing apparel' than do men, and that men exceed women in naming utensils and implements, professions, and abstract terms. However the Wellesley results do not agree with these findings. The results of Miss Manchester (6) in California show more agreement with the findings of Jastrow with reference to sex differences. However, Miss Tanner (5) raised the pertinent question as to whether these tests indicate native differences or acquired traits. She thinks that lists written by college students will be affected by recent experiences in college study and activities. (See also Whipple (7).)

Fifty-six full-blood Indians were given the 'continuous association' experiment in which they were told to write as many words as they could in three minutes.[1] 141 white students were given the same experiment; 56 of the latter were taken at random for comparison with the Indians. In both groups the sexes were equally divided and the school grades ran from eighth through tenth grade, the grades being equally represented in the racial groups. The 85 additional whites were used as a further control.

The record sheets on which the students' reactions were written under the test conditions were taken in hand and the 'ideas' were classified by one person into the categories used by Jastrow and others. These will be found in the accompanying Table I.

The median age of the Whites of Group I is 14.7, Q 1.4 years; of Group II median 15.8, Q 1.0 years; and of the Indians it is 18.1, Q 1.0 years.

In order to make the comparison of the community of ideas of full-blood Indians with that of the Whites we expressed the number of words occurring in a category for each subject as a per cent of his total score. The per cents were then added and are presented as total per cents for the groups for a category. (See Table I) In handling the data

[1] The writer is under obligation to Mr. W. A. Francis, M.A., of the Department of English of the University of Texas, for assistance in tabulating the responses of the subjects of this experiment.

we wish to know first, whether the Indians preferred one category above another in any sense differently from the white subjects. Next, we would like to ascertain if the behavior of the sexes is different or similar in this same respect of preference of category.

TABLE I

COMMUNITY OF IDEAS OF 56 FULL-BLOOD INDIANS, A GROUP OF 56 WHITES (I.) AND A GROUP OF 85 WHITES

Categories	Per Cents Whites			Per Cents Indians			Per Cents 85 W. on basis 56 Cases	Zero Scores	
	Boys	Girls	B.&G.	Boys	Girls	B.&G.		W.	I.
	A	B	C	D	E	F	G	Fre.	Fre.
Animal Kingdom	253	257	510	187	329	516	540	6	9
Wearing Apparel	94	90	185	86	99	185	226	20	22
Proper Names	133	123	257	47	27	74	142	32	41
Verbs	482	503	986	667	679	1,337	1,160	3	0
Implements and Utensils	110	104	214	227	338	565	328	14	11
Interior Furnishings	69	96	165	91	148	239	162	14	19
Adjectives	373	262	636	226	163	389	478	2	14
Foods	53	107	160	67	80	147	72	24	31
Vegetable Kingdom	67	63	130	67	69	136	77	24	25
Abstract Terms	66	99	165	66	42	108	186	20	32
Buildings and Materials	84	153	237	173	114	287	200	19	20
Parts of Body	105	109	215	75	60	135	151	24	29
Miscellaneous	69	55	124	47	29	76	65	25	36
Geographical and Landscape	166	102	269	92	26	118	196	14	28
Mineral Kingdom	9	21	30	34	25	60	10	38	41
Meteorological, Astronomical	47	43	90	33	35	69	53	29	38
Stationery	57	112	169	53	49	102	112	20	31
Occupations and Callings	18	6	24	36	13	49	31	44	45
Conveyances	65	34	100	65	5	70	79	29	41
Educational	180	201	381	135	293	428	847	9	19
Other Parts of Speech	257	220	478	229	184	413	320	11	18
Arts	7	8	15	14	1	14	22	45	54
Amusements	4	6	10	17	1	17	49	47	54
Mercantile Terms	13	13	26	43	1	43	60	41	47
Kinship	19	13	32	23	1	23	66	40	55
Total	2,800	2,800	5,600	2,800	2,800	5,600	5,600	594	760

Table I shows what per cent of a group's responses fall in a category. For example, 256 times out of 100 the 28 boys on a basis of 100 words each, a total of 2,800 words, chose words falling in the category of animals. This is a little better than one out of ten. But the case of verbs (6) is strongest of all for the White boys, being 483 out of 2,800

or nearly one out of six. The next strongest category with the boys is that of adjectives. It will be noticed that the White girls subjects are disposed in the same manner as the boys excepting they are slightly stronger in their tendency to use verbs than are the boys, yet we have the same sequence with respect to these three categories—verbs, adjectives, and animals. As respects the Indians, the boys are more disposed to give verbs than are the Whites, the number here is about one out of every four, *i.e.*, nearly one fourth of their words fall into this category. The next strongest category with them is that of implements and utensils (if we disregard the category 'other parts of speech' for the boys, which is not a very significant category). Next for the Indian boys it is adjectives and for the Indian girls it is animals. As to both groups, Whites—56 boys and girls—and Indians—56 boys and girls—the strongest category is again verbs, being for the White boys and girls combined one out of a little more than five, and for the Indian boys and girls, one out of about four. The Indians seem more disposed to give verbs than the Whites. For the total groups, the Indians next give implements and utensils, then animals. The Whites give adjectives, then animals the preference. In both groups the category 'educational' is rather strong, being (if we disregard again the category called 'other parts of speech' for the Whites) fourth in the sequence of preference. The second group of Whites, 85 in number, show the same inclination as do the first group of Whites, 56 in number, and Indians to give the greatest preference to verbs. Here it is about one out of five. The category of next greatest popularity is 'educational terms,' about one out of six. Next for this second group of Whites is animals, about one out of ten.

It is evident in all sex and racial groups that the categories given in italics in the Table I are the most significant. They are in order: verbs, animals, adjectives, educational terms, and geographical and landscape features, and buildings. Interior furnishings are seen to be stronger in the Indians than in the Whites.

A comparison of the facts of Table III will show that in

all the categories taken together, those significant and those not very significant the Indian and the White group were just about the same, if not the same, in 7 out of the 25 categories. In 9 the Indians were excessive as compared with the Whites and in 10 they were behind. This might indicate race differences on its face, but when we look at the comparison of this same group of Whites with another White group (Group II) we find they are just as different from the first White group as are the Indians. So we are inclined to say the difference cannot be racial, as here indicated in the first instance.

As to sex differences a study of Table I indicates that the Indian boys and White boys have measures about the same in about five instances or categories and that the Indian girls are a little more inclined to differ from the White boys in the size of the various category measures, and that the White girls are more inclined to be like the White boys (there are seven category numbers which are about even). But the Indian girls are not as much like the Indian boys as are the White girls like the White boys—the sex similarities being stronger for the Whites than for the Indians; however there are sex differences as here indicated in both race groups. But this does not take into consideration the facts of sequences in category preference. It only considers in a very general way the respective sizes of the categories considered.

TABLE II

SHOWING RATIOS OF THE GROUPS FOR THE SIGNIFICANT CATEGORIES

Category	Ratios Totals Ind. to W. Gr. I	Ratio Whites		Ratio Ind. Boys to W. Boys	I. Girls to W. Boys	Ind. Girls to W. Girls	Ind. Girls to Ind. Boys
		Gr. II White	Girls to Boys				
Animal Kingdom	100	100	101	73	130	128	176
Wearing Apparel	100	123	104	90	104	110	115
Proper Names	28	54	91	35	20	23	57
Verbs	135	117	104	138	139	133	100
Implements and Utensils	273	140	94	206	307	325	149
Adjectives	61	75	70	60	43	62	72
Buildings and Materials	121	85	102	206	135	74	65
Parts of Body	62	71	102	71	56	55	80
Geographical and Landscape	40	73	61	53	155	25	28
Educational	112	222	112	75	163	146	217

Table II will show various ratios pertaining to the categories when one sub-group, racial or sex, is compared with another. In the first column we have the ratio which obtains when the total Indian group is compared with the first White Group I of fifty-six. We find the categories 'animal kingdom' and 'wearing apparel' are of equal prominence here, that the numbers for the category 'educational' are about the same for the two groups. 'Buildings' and 'materials' are slightly higher for the Indians than for the Whites, somewhat higher is 'verbs' for the Indians. But when we compare the two White groups, the situation is no better. They are just as different from each other as are the Indians from the first White Group I. However, the White boys and girls are more similar in that there are more categories whose ratios approach 100 when we compare the sex groups, *i.e.*, five out of ten are 100. In fact all but two are fairly close to 100. These exceptions are the categories 'adjectives,' and 'geographical landscape.' The Indian boys are quite different from the White boys Group I as determined by the ratios of the categories for in only one instance is the ratio 100 or anywhere nearly that. From the standpoint of ratios the Indian girls are slightly more like the White girls than are the Indian boys like the White boys. In some instances, we see, the ratios indicate race differences and in some, none. The results are not consistent and since they are not we should not be justified in saying that from this standpoint of ratios, race differences are indicated. The Indian group is more similar to the first White group than is the second White group. This throws some doubt on evidence found in the strong similarity between the White boys and girls. Again we should expect a stronger similarity between the Indian boys and girls. They are less alike than the two racial groups. Certainly this is a disappointment to one looking for racial differences in community of ideas between Indians and Whites.

We now consider the sequence of the categories. In order to do this, we have had recourse to the Pearson Rank Differences method of Correlation. Reference to Table III

TABLE III

SHOWING CORRELATIONS FOUND BETWEEN THE SEQUENCE OF THE VARIOUS CATEGORIES FOR THE VARIOUS SUBGROUPS

For White Boys and Girls	r is + .88, P.E. ± .03
For White and Indian Total	r is + .88, P.E. ± .03
For Indian Boys and Girls	r is + .88, P.E. ± .03
For White and Indian Boys	r is + .91, P.E. ± .024
For White and Indian Girls	r is + .83, P.E. ± .042
For White I and White II	r is + .91, P.E. ± .024
For Zero Scores for White I and Ind	r is + .94, P.E. ± .017

The differences in the correlations that are different according to the formula $P.E._{(diff.\ r_1-r_2)} = \sqrt{P.E._{r_1}^2 + P.E._{r_2}^2}$ are of such little significance as to indicate no difference when these are found.

will reveal that in so far as the sequences of the categories for the several racial and sex subgroups are concerned they show very high correlation. These range from .83 to .91 and are all positive. The correlation between the White Group I and the total Indian Group is the same as that for the White boys and girls, + .88. The White and Indian girls show a correlation for the sequence of categories of + .83. But this is so little different from any other correlation that application of the formula for showing a difference in correlations reveals no actual difference of significance. Consequently this test of comparison of sequence of the categories reveals no difference, either racial or sex.

It was thought worth while to see what categories were omitted by the White Group I and the total Indian group, that is, what categories showed certain frequencies of zero. (See Table I) The Indians were more inclined to omit any of the categories than were the Whites as only one Indian used a term indicating kinship and only two used a term indicating amusements or arts. But 'arts' was the most infrequent category with the Whites who failed in 47 instances to give a word that might be so classified. In fact the categories generally which were unpopular with the Indians were unpopular with the Whites. The correlation between these zero scores of the Whites and Indians is rather high for the categories; it is + .94, P.E. ± .017. This is merely another way of ascertaining whether the racial groups are similar

or different in their associative processes as indicated by the records; and they are found to be similar in the tendency to use certain categories lightly, or the absence of members of a category.

Summary

1. When White and Indian children are given the Continuous Association Test and the records of these are compared we find them rather more similar than different when the recorded words are classified into categories.

2. The strongest category for both Whites and Indians is that of verbs, though the Indians are more disposed to give verbs than are the Whites of Group I.

3. The Whites of Group I are more disposed to give adjectives than are the Indians.

4. The Indians prefer slightly to give animals more than the Whites of Group I.

5. In nine categories the Indians were excessive as compared with the Whites of Group I and in ten, behind them.

6. But the Whites of Group II are found to be just as different from the Whites of Group I as the Indians are found to be.

7. The White boys and girls seem to be more similar to each other than do the Indian boys and girls.

8. From the standpoint of ratios, race differences are not indicated, for here the Indians are more like the Whites of Group I than are the Whites of Group II.

9. From the standpoint of correlation of sequences of the categories for the several groups, the measures of correlation are high for all groups whether racial or not and any difference in correlation found is of no significance as indicated by measures of difference.

10. Correlation of tendency to slight a category is very high for the Indians and Whites. Both are similarly inclined to slight the same categories.

11. In community of ideas Indians and Whites are not found to be different in so far as these data are concerned.

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